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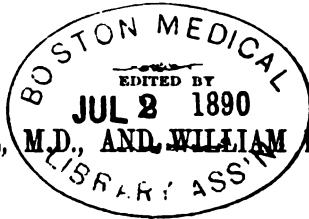
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THE

GLASGOW

MEDICAL JOURNAL.

VOLUME NINTH.



JOSEPH BELL, M.D., AND WILLIAM LEISHMAN, M.D.



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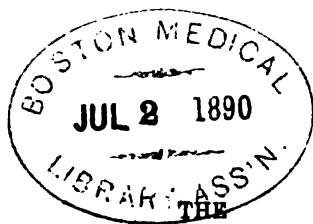
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ORIGINAL COMMUNICATIONS.

- I.—*Illustrations of Puerperal Diseases.* By R. UVEDALE WEST, M.D., Fellow of the Royal College of Surgeons of Edinburgh; Vice-President of the Obstetrical Society of London, &c., &c.

WHAT is puerperal fever? M. Cazeaux, in the course of an admirable speech before the French Academy of Medicine in April, 1858—a speech in which he gives not only his own opinions on the subject, but also a very good digest of the different and contradictory opinions held by previous speakers in the debate—observes that, “here we have to do with a disease which presents itself with lesions the most varied; sometimes a metritis, sometimes a peritonitis, sometimes a phlebitis; at other times a lymphangitis, a pneumonia, a pleurisy, a suppurating arthritis,” &c. Further on he dwells on the fact that we have usually an anæmic and even an uræmic condition in the puerperal state, and he goes on to say that “it is in the blood, so profoundly modified in pregnancy, that we must seek the first cause—the *point de départ* of puerperal diseases;” and, a little further on, he says, “there is, therefore, in lying-in women a great morbid aptitude, which I will at once call a *pyogenic state*, which not only manifests itself in abdominal phlegmasies, but makes its sad influence felt, whatever be the disease attacking the puerperal female.” Puerperal fever, therefore, is not one and indivisible. As Dr. R. Ferguson says in the essay prefixed to the New Sydenham Society’s edition of Gooch’s invaluable work—“Gooch discovered that puerperal fevers were many, requiring varying modes of treatment.”

The following group of cases illustrates to some extent the truth of the preceding remarks; they show that even convulsions and mania, occurring in the puerperal state, if they may not be exactly regarded as puerperal fevers, may either be associated with, or degenerate into, a form of disease which must, in the extended sense of M. Cazeaux, be regarded as a puerperal fever. In Case I., which I offer with such observations as suggested themselves to me at the time, it will be seen that I express some surprise at the seeming want of pathological connection between the different states, present together, or following one another. That surprise I no longer feel, enlightened by the doctrine of Cazeaux, as well as by the observation of subsequent cases, such as Cases II. and III., and others which will follow:—

OF CEREBRAL EXCITEMENT AND DISORDERS OF THE MIND IN THE PUERPERAL STATE, REGARDED AS VARIETIES OF PUERPERAL FEVER.

CASE I.—CONVULSIONS.

Mrs. J. B., the wife of a draper in Alford, was prematurely confined of a living child on the 6th February, 1854, and so hastily that she was very near being alone when delivered. It was her first labour, and she was greatly agitated at this so sudden event; so much so, that very soon after the removal of the placenta by her medical attendant, who was on the spot as soon as possible, she went off into strong convulsions. Mr. O. bled her largely, and the convulsions ceased. She had suitable medicines administered afterwards. For a few days she had blindness and deafness, and with the exception that she had occasional diarrhoea, and that her pulse never fell below 120, for a fortnight she appeared to recover, and got into her sitting-room. At the end of that time she had alarming faintings, for which brandy and other stimulants were administered. On the 24th of February, Mr. O. requested me to see her with him. She had had a very distressing cough for the previous twenty-four hours, with pain low down in the left side. The pulse was 140, with a feverish flush, most remarkable in one cheek. She had had some expectoration, but no sputa were forthcoming for our inspection. Auscultation, however, made it quite clear that she had pneumonia. The patient having been freely depleted so recently, and having also suffered from faintings a few days previously, we thought leeching and antimonials scarcely admissible; we therefore applied a blister, and administered calomel and opium in pills every four hours, one grain of the former with a quarter of a grain of the latter. The blister

relieved the pain at once; and after she had taken the pills a day or two, the pulse being below 100, Mr. O. omitted the opium, thinking the bowels were becoming too constipated. Unfortunately diarrhoea came on again, which resisted the ordinary astringents as long as we persevered with the calomel. We, therefore, discontinued that medicine, the pneumonic symptoms being apparently relieved, there being no pain and scarcely any cough. Still the pulse was continually getting up to 120 again, particularly in an evening, and there were night-sweats occasionally, a state of things which indicated a hectic condition, for which, as well as for the still obstinate diarrhoea, we gave sulphuric acid, with sulphate of quinine and sulphate of copper. This did not answer as we wished, the diarrhoea continuing; so we gave decoction of oak-bark with catechu and laudanum. The diarrhoea now abated somewhat. On the 5th of March, during an evening visit, it was noticed that she coughed slightly. She was questioned more closely about the cough, which she had for several days denied having suffered from at all, as well as that she had had any pain. Some sputa were produced in reply, which were decidedly pneumonic; and auscultation again revealed the pneumonia in the old place. A blister was again applied, and the calomel and opium commenced again, along with decoction of oak, catechu and cinchona. The rapid pulse in an evening, the progressing emaciation, and the tendency to hectic, were all accounted for by the existence of pneumonia in a latent and somewhat chronic form. Under this treatment the sputa gradually improved in appearance, the pulse got below 100 again, and on the 12th I ceased my consultation visits, Mr. O. reporting to me from time to time that she was improving, that her pulse a few days after was as low as 88, and that she had once again got into her sitting-room. The calomel was discontinued, and this apparent improvement went on until the 20th of March, when phlegmasia dolens of the left leg came on rather suddenly, with great pain, swelling, and a hard swollen condition of the femoral vein. Mr. O. applied leeches inside the thigh, fomented the part, &c., and the next day I saw the unfortunate woman again. It was a decided case of phlegmasia dolens. The pain was relieved greatly, but the swelling and the hardness of the femoral vein remained. The pulse was 130. There was considerable emaciation. The cough and mucous expectoration had entirely ceased on the accession of this new disease. Friction with camphorated oil was used to the leg, with opiates at bedtime. On Sunday the 26th of March she suddenly coughed up four or five ounces of pure pus, which Mr. O. brought to show me. A vomica evidently had burst, proving that the pneumonia had not been cured after all. She now got

rapidly worse, her alvine evacuations became very frequent and involuntary, and when I saw her once again, on the 28th, her pulse was 160, and her respiration 50 in the minute. She died at midnight on the 29th, having survived her confinement fifty-one days. It was the unfortunate occurrence of obstinate diarrhoea which interfered with the effect of the calomel. It was singular that the pneumonia should have been so masked throughout. The gums were never affected. The sudden appearance of phlegmasia dolens, so long after the confinement, and without the usual premonitory symptoms of abdominal or inguinal inflammatory tenderness, was rather remarkable. There was no mistake about the pneumonia having been the only inflammatory condition present.

The convulsions in this case appeared to have been caused by mental excitement. But I cannot help remarking on the strange sequence of diseases in the case—convulsions, followed after an interval of several days by pneumonia; that disease masked and accompanied by obstinate diarrhoea; the subsequent attack of genuine phlegmasia dolens, so very long after the confinement, when *uterine* irritation might have been supposed to be quite at an end; and lastly, the fatal development of the pneumonia into suppuration. Speaking of some of the sequelæ of convulsions, Dr. Ramsbotham says:—"Denman mentions that in almost every case he had seen, there was evidently after delivery a greater or less degree of abdominal inflammation; Collins has found a strong tendency to peritonitis, even when blood has been taken freely; and Gooch gives a case exemplifying the truth of these observations. Although it has happened to myself to meet with a few instances of peritoneal affection subsequent to puerperal convulsions, the number has not been by any means so great as to impress my mind that the latter disease had any connection with the former; nor would it have occurred to me to imagine such a connection, had the remark not been made by high practical authorities."*

It is, however, quite certain that the convulsions had, in the case I have here given, left a morbid impression on the system, or why should the pulse have continued so frequent? In a paper on the subject of puerperal convulsions by Dr. Cormack (*London Journal of Medicine*, 1849), a case of convulsions during labour is given, in which phlegmasia dolens came on three days after the delivery. It is not stated whether this attack was preceded by the usual symptoms of abdominal or uterine inflammation; probably there would be something of the kind. Phlegmasia dolens may, therefore, be looked upon as one of the sequelæ of convulsions. But in the case I have here given, that disease

* Ramsbotham, *Obstetric Medicine*. Third edition, p. 471.

would appear to have been the last of a series of diseases having no recognized pathological connection one with another, and occurring very long after the labour. Would the diarrhoea constitute a sufficient source of irritation to cause phlegmasia dolens? I once had a case of peritonitis in which diarrhoea came on, and afterwards phlegmasia dolens, after a considerable interval.*

CASE II.—MANIA.

Mrs. C. of A., near Alford, a woman of very full habit, was confined early in the morning of the 2nd of October, 1858. She had a dose of ergot given her in consequence of *inertia uteri*, and was delivered about twenty minutes after. It was her eleventh confinement, and although she had always suffered severely from afterpains, she had every time recovered without a bad symptom. On the present occasion, she had some feverish symptoms on the third day (October 4th), for which I gave her sulphate of magnesia, with Dover's powder. On the 6th and 7th she appeared somewhat better; but on the evening of the latter day, her husband, a small farmer, addicted to drink, came home late much intoxicated, and quarrelled with the nurse, who, he said, was drunk! The nurse ran off to a neighbour's house and did not return. I was summoned at 6 the next morning (October 8th), and found my patient in a very excited hysterical condition, with a thin pulse about 120. I sent her a few doses of chloric æther in camphor mixture. The same day, in the afternoon, I had a hasty summons. I found my patient quite maniacal, very boisterous, and with many delusions; her pulse 120 as in the morning, but softer and fuller; her milk going, with other untoward symptoms. I sent her a draught containing a grain of acetate of morphia to be taken at bedtime.

October 9th, manè.—Has many delusions, but is not so boisterous; she has not slept; her pulse is 112. Continue mixture. *Vespere.*—Is better; has no delusions; has not slept, but feels sleepy; pulse 114. Bowels have been well moved with castor oil. Repeat the night-draught.

* This was the case:—"Labour severe and protracted from a thick and rigid state of the os uteri. After-pains severe for several days without acceleration of pulse. On the ninth day symptoms of peritonitis came on; and, when I saw her the next day, there was exquisite tenderness of the abdomen, with a pulse 150, small and sharp, with the other usual symptoms. I bled her in the arm, and in two hours the pulse dropped to 120, the pain being greatly relieved. I leeches the abdomen, gave calomel and opium in frequent doses, &c. A week after, the symptoms of peritonitis being quite checked, diarrhoea, with subsultus tendinum, came on, but the pulse did not get higher than 120. I gave chalk mixture, with carbonate of ammonia, aromatic confection, wine, &c., which speedily removed these symptoms. At the end of the month a smart attack of phlegmasia dolens came on, which was readily subdued by leeching the inside of the thigh affected."—*Note in Register*, December, 1837.

10th, *manè.*—Has slept well; is quite cool and collected; pulse 80. Continue mixture, and repeat the night-draught.

11th, *manè.*—Has not had so good a night, and has slight delusions. This morning she talks volubly of events which she persisted had occurred the day before, but respecting which she was altogether mistaken, and has a good deal of quiet conversation about events of twenty and fifteen years ago, about which her memory is correct, but all which are apropos of nothing at all. Pulse 98. Lactation checked, but not entirely suppressed; tongue whitish; complains of a ringing in her ears. Repeat the night-draught.

12th—No sleep last night; is however better, being perfectly quiet and rational. Complains of pain in legs; tongue covered with yellow fur; pulse 95. Continue mixture, and repeat night-draught.

R. Pil. hydr.

Ext. hyoscyami āā. gr. xij.

Pulv. rhei. gr. vj.

M. Ft. pil. vi.

Sumat ij. omni nocte h. s.

13th—Was rather delirious last night, but slept rather better, and is now very quiet; pulse 98; complains of pain in all her limbs, and of indefinite uneasy sensations. Continue mixture, and repeat night-draught.

14th—Is rather feverish; pulse 114. Is just now rational, but thought last night she was bewitched. There is less pain complained of in limbs, and the tongue is cleaner. Continue.

15th—Is very quiet; is not so feverish; pulse 98; tongue furred; was purged a good deal yesterday; is very flatulent; does not now complain of pain in limbs. Repeat night-draught.

R. Spir. ammon. foetid. ℥ss.

Infus. gent. co. ℥vij. M.

Sumat coch. ij. quartis horis.

16th—Great tympanitis with occasional spasmodic pain in abdomen; pulse 104; is quite collected. The bowels not having been moved at all since yesterday morning, I gave her a dose of castor oil. Repeat night-draught.

17th—Although she did not take the night-draught as ordered, she has had a good night; yesterday afternoon was very talkative and delirious, singing occasionally; this morning is very quiet and still, and quite rational. The castor oil given yesterday has operated several times. There is still great tympany; tongue rather cleaner; pulse 98.

18th—(In consultation with Dr. Grantham). Is better; has had a good night; was not so delirious last night; tongue

cleaner; tympany as before; some tenderness in both groins; lies with knees up; bowels open; pulse 80, soft.

Sumat. statim.	Ol. terebinth.	℥ij. ex lacte.
R. Quin. sulph.		gr. x.
Acid. nitric. dilut.		℥j.
Aquæ		℥viij. M.
Sumat coch. ij.	quartis horis.	

19th—No delirium last night; is now perfectly quiet and rational; cool and pale; groins still tender; knees drawn up; is purged a good deal; pulse 80. Continue quinine.

20th—Much better in every respect; knees down; diarrhœa abated; appetite returning; pulse 76. Continue.

21st—Nearly well; appetite good; no pain; tenderness gone from left groin, but still felt slightly in right; pulse 76.

22nd—A smart shivering fit last night at 6. Fever and restlessness without delirium all night. This morning her knees are drawn up again; tenderness in right groin; fever; headache; pulse 108.

R. Spir. æth. nitr.	℥vj.
Liq. ammon. acet.	℥ij.
Tinct. card. comp.	℥j.
Mist. camphoræ ad	℥viij. M.
Sumat coch. ij.	quartis horis absente febre.

To take the quinine medicine only in the intermission.

23rd—Has had a good night; had a rigor this morning at 6. Is now, 11 a.m., rather feverish; pulse 114; is more cheerful; knees drawn up; groin tender. Continue. Foment the groins.

24th—Fever all night, which subsided at about 9 this morning; had a smart rigor at 11. Pulse now (*noon*) 120; great heat of surface; no delirium; right groin still tender. Gave her some saline medicine in an effervescent form.

25th—Less fever; but calf of right leg is swollen; and very tender to touch; no appetite; pulse 100. A poultice to calf.

26th—Is better; no fever; pulse 90. Suppuration commencing in calf; is very deaf. Continue quinine, &c. *Same day, 9 p.m.*—Husband having come home drunk from market, she had some kind of fit which I did not witness, but during which she could not speak. I was summoned; I found her better again, but breathing very hard and complaining of great pain in right side of chest; sound on percussion there dull; pulse 88. Mustard poultice to side.

27th—Was summoned in the night. Saw her at 2 a.m. Has just had another fit as last night; respiration worse; pain in side the same; has a cough, and has expectorated some rusty

sputa; there is crepitation; is rational; skin warm and moist; pulse 84. Blister to side.

11 a.m.—Hearing better; respiration easier; has expectorated rusty sputa several times; abscess in calf softening; pulse as before. *Evening.*—Is much exhausted, having been out of bed; breathing no worse; no fever; pulse 102, very weak. Wine *ad libitum*.

28th—Is better; nice soft pulse, 90; appetite returning; pain in side better; cough gone; abscess in calf advancing. Continue quinine.

29th—Feels altogether better; pulse natural, 72; still some pneumonic sputa. Opened the abscess.

30th—Slight cough; no bloody sputa; abscess discharging; appetite good; pulse 80.

November 1st—Very feeble, but does not look so anæmic as she did; abscess discharging; no rusty sputa, but still a slight cough; cannot lie on right side; great tenderness to touch on inside of knee, but none higher up the thigh; whole of leg swollen and tender to touch, especially in inside; appetite good; tongue clean; skin natural and open; pulse weak, soft, 90.

2nd—Fever of hectic type; pulse 100; appetite very good; less pain in legs.

4th—Right groin still tender; no fever; pulse 90; appetite good; abscess discharging. Continue quinine. Camphorated oil to groin.

9th—Coughs slightly; pulse 80. To take cod-liver oil.

24th—I had discontinued my attendance, trusting that the abscess in the calf of the leg had eliminated the *materies morbi*; and that there would, consequently, be no further relapses; but I was deceived, for I was summoned again this day, and found a large abscess on the right buttock, which was advancing to suppuration, having begun to inflame a fortnight before. I ordered poultices.

28th—Opened the abscess on the buttock. Complains of acidity, eructations, and other dyspeptic symptoms, with occasional shiverings.

R. Potassæ bicarb.	℥ij.
Magnesiæ calcin.	℥j.
Tinct. card. comp.	℥ss.
Spir. ammon. aromat.	℥ss.
Aquæ ad	℥viiij. M.
Sumat coch. ij. ter. in die.	

I advised the patient to keep the abscess discharging as long as possible, for I thought that that of the calf had healed up too soon.

From this date the patient had an uninterrupted progress to recovery, and on the 31st of October, 1859, was confined of twins after a very rapid labour, from which she recovered without a bad symptom.

CASE III.—MANIA.

Mrs. T. of H., near Alford, the wife of a sailor who was absent at sea, a healthy-looking young woman aged 24, of very excitable temperament, was confined on the 4th of August, 1860. It was her first labour, and was rather hard, with first position of cranium. She had a dose of ergot, and the child, a large, living male, weighing 9 lbs., was brought away with the vectis. She went on well, having no trouble with lactation, until the seventh day (Saturday, August 11th), when she had a smart rigor, followed by fever and sweating; pulse 140; no pain. She had an aperient, and some doses of Dover's powder. When I saw her next day (Saturday) she had a very hot skin, with profuse perspiration; no pain; bilious tongue; lochia and milk sufficient. The bowels had been well moved; pulse 130.

R. Magnes. sulph.,	℥i.
Pulv. ipecac. comp.	℥ij.
Spir. æth. nitric.	℥ss.
Mist. camph. ad	℥viij. M.
Sumat coch. ij.	quartis horis.

To take 4 grs. of blue pill every night for three nights.

Sunday—Still hot skin, with perspiration; pulse 120.

Monday—In the afternoon, is sitting up; is quite cool; pulse 100; says she feels well. The same night at 11 had another rigor, followed by high fever, with hot skin and hysterical (maniacal?) delirium all night. This state continued throughout Tuesday, with profuse hot perspiration. Continue medicine, and take a grain of acetate of morphia at bedtime.

Wednesday, a.m.—Has rested well, is now cool and free from fever; milk going; lochia degenerating into "green waters," but she has been confined twelve days or thereabouts. Continue medicine.

Thursday, a.m.—Fever came on again last night after another rigor. Pulse now 120; skin moist and cool; tongue bilious; no anxiety of countenance; no hurry of respiration; bowels well open; abdomen much distended and tympanitic, no pain or tenderness to touch; no pain in head. (Query—an ague?) Sumat quin. sulph. gr. iij. tertiis horis absenti febre. Repeat morphia at bedtime.

I was sent for again at midnight; she had been furiously delirious (maniacal?); but on my arrival, having had cold

vinegar applied to her forehead, was quiet again; skin cooling down; pulse 96, soft and pleasant. She had diarrhoea to some extent; abdomen still very tympanitic; had not taken her night draught. I gave her her draught, and sent her *Mist. cretæ comp.* To take an ounce after each loose stool, and omit the quinine until the bowels were better.

Thursday, August 16, a.m.—Is quiet and nearly rational, but now, 11 o'clock, the fever-fit is again coming on; pulse 125, and thinner; diarrhoea going on, with great tympanitis. *Evening, 8 o'clock*—Feverish; dry skin; flushed cheeks; no anxiety of countenance; no hurry of respiration; pulse 130; bowels have been moved three times since the morning; tympany still present; there is milk in the breasts, but sparing and watery; appetite not quite gone. Pergat in usu *Mist. cretæ*. Sumat haustum morph. h. s.

Saturday, 18th, 11 a.m.—Is sitting up in bed, and seems better in every respect; skin cool; no flush of countenance; pulse 112; appetite moderate; has just had a very healthy formed motion. But I do not like the obstinacy with which she persists in sitting bolt upright in bed, without pillows or support of any kind to her back. Moreover, she has had all her bottles of medicine before her on the bed, and has been puzzling herself for more than an hour in trying to decide which is the right medicine to take. She talks much about her finger nails, and expresses much anxiety respecting *my* conversion, a neighbour, who is a Wesleyan, having been praying with her. In short, there still seems to be a maniacal element in the case. She is as tympanitic as ever, without pain or tenderness. To resume the quinine in 2 gr. doses, and with *Pil. galb. comp.* in 5 gr. pills. I had some little hope that she might do well, but when I called on

Sunday morning, 19th, I found her worse than ever—quite maniacal. Her husband had arrived in the night, but she took no notice of him. She talked incessantly and very incoherently, her subjects being chiefly delusions as to her own state—that she was full of water, &c., &c. She was chewing her saliva until her mouth overflowed with foam; the pulse was 140; the skin hot and very sodden with warm perspiration. I gave her a turpentine enema, hoping thus to relieve the tympany. *The same evening*—She was cooler, and perhaps not so turbulent; delusions as in the morning. She died in the course of the night.

CASE IV.—METRITIS DEGENERATING INTO HYSTERIA, OR
HYPOCHONDRIASIS.

Mrs. F. B. of A., near Alford, a remarkable healthy looking woman, the wife of a respectable farming-man, and the mother of

several children, was confined on the 11th of April, 1859, after a very easy and quick natural labour. Calling on the patient on the fourth day, April 14, I found her with a nasty, small, sharp pulse, 120; abdominal tenderness; *fundus uteri* large and very tender to the touch; lochia, as well as milk, abundant. She stated that she had had a rigor the day before, and that the headache she now suffered from had been greater on that day. I ordered fomentations and a dose of castor-oil. I found her much better the next day. But on the 19th I received a message. I found her with gripings and looseness of bowels; the pulse more than 100; no abdominal tenderness; lochia and milk abundant; appetite good.

R. Confect. aromat.	℥ij.
Tinct. opii	℥j.
Spir. ammon. aromat.	℥ss.
Aquæ menth. pip. ad	℥vj. M.
Sumat coch. ij. quartis horis.	

I heard no more until May 1, when I found her sitting up, with a small pulse, 130, complaining of pain at *scrobiculus cordis*, probably from indigestion, although the appetite was but little impaired, and the bowels are comfortable. The milk was scanty, and the lochia watery. Somehow she was not "getting on."

R. Quin. sulph.	gr. x.
Acid. hydrochloric.	℥ss.
Aquæ ad	℥vj. M.
Sumat coch. ij. ter in die.	

May 6th—Better; pulse 86. Continue quinine.

7th—Not so well again this morning. Is now, 4 p.m., complaining of pain at sternum; is very flatulent with pyrosis, and short dry cough; bowels loose again. Pulse 110. There is dyspepsia and intermittent fever. She complains of her skin feeling hot, though to my touch it certainly is not so. I think she is fanciful about herself. To omit the quinine for a few days, and to take a few doses of bismuth, chalk, and infusion of gentian.

13th—Is better, but very depressed in spirits. She has hysterical urine; the pulse 80, weak; eats well and sleeps well; she has many absurd fancies; thinks her milk is all gone, but that is not the case.

And so she went on for many weeks. It was undoubtedly a case of metritis in the commencement, followed by a slight affection of the mind. She menstruated about the 1st of June, but still went on with her fancies, not entirely getting rid of them until she again became pregnant. She was confined again on the 28th of January in this year, and is now quite well.

Cases I. and II. in the group described above, afford examples of the influence of the *pyogenic state* referred to by Cazeaux, on cerebral disturbance in very excitable subjects, under special circumstances calculated to excite them. In both of them there was pyæmia, in both of them pneumonia. Case I. terminating fatally after protracted sufferings, while Case II. did not recover until after perfect elimination of the purulent *materies morbi*. In Case III. there was no evidence of a pyogenic state, unless the frequent rigors may be regarded as such evidence; but there was diarrhoea, great tympanitis and troublesome fever going on, *pari passu*, with the maniacal condition which was the prominent feature in the case. It was, certainly, not the mere delirium of fever. In Case IV. a genuine puerperal metritis degenerated rapidly into a very tedious and tiresome hypochondriacal condition.

(To be continued.)

II.—*On Infantile Death Rates, in their Bearing on Sanitary and Social Science.* By W. T. GAIRDNER, M.D., Fellow of the Royal College of Physicians of Edinburgh, &c., &c.

(Read before the National Association for the Promotion of Social Science.)

EVERY one practically engaged in the study of the public health is aware of the great value justly attached to the death rates of young children, as indicating the favourable or unfavourable sanitary position of a community. Indeed, it is too obvious to require proof, that whatever may be the local circumstances surrounding the life of the adult, the ability to rear a healthy offspring in sufficiently large numbers is one of the first conditions of national existence. The community which, either from non-production, or from the production of sickly and fragile children, fails to bring its full quota to the stock of life necessary for the national well-being, is not in a satisfactory position. It has ceased to be self-sustaining, and is either declining in numbers and vigour, or it is maintaining itself at the expense of other communities more favourably placed. Nay more, a community thus degenerating is in all probability always a cause of more or less active mischief to others, and so to the nation at large. In so far as it produces diseased lives, or lives prone to disease, it tends to infect the healthy stock of the nation with corresponding diseases and liabilities. No sanitary arrangements can prevent

the unhealthy members of a society or of a nation from filtering into the general community, and becoming the parents of a generation tainted with hereditary weakness. The presence, therefore, of a degenerating community among others more happily organized, is not a mere deficiency to be compensated out of the superfluities of the rest; it is a positive evil—or rather, the source of a multitude of evils to society at large.

A simple and easily-applied test of the soundness or degeneracy of the race in any locality would be of infinite value to the sanitary and social reformer. But such a test is not afforded by the mere increase in numbers of the population, for it is well known that in the great centres of commercial and manufacturing activity there is never wanting an abounding supply of new, though not always vigorous life, to fill the places of the lives prematurely destroyed. Nor is such a test to be found in the general death rates, which are far too complex in their character, and subject to too many extraneous influences, to show adequately the degree of prominence of the evils referred to. The test is only to be found, if at all, in the more careful examination than hitherto of the infantile death rates.

The importance of infantile death rates, as indicating the social and sanitary condition of a community, depends on two considerations; first, that infants are much more easily affected than the general community by most of those causes of disease and death which are common to all; and, secondly, that their dependence upon their parents for bodily organization, as well as for proper nourishment and support subsequent to birth, makes the sanitary state of very young children a most delicate test of the real health and well-being of the parents, *i.e.*, of their social and moral condition at the productive period of life, and in so far as concerns the domestic relation. In other words, the proposition may be put thus:—Given a community in which the infants die with extreme rapidity, and in which the general death rate is also high, you have, in the fact of the high infantile mortality, not only a corroborative proof of defective sanitary conditions operating on the entire population, but, in proportion as the infant mortality is higher than the average of places having the same general death rate, you have proof of defective sanitary conditions operating specially on the young life, in all probability through the neglect, or vice, or ignorance of the parents, and through their failure to fulfil the necessary conditions of a sound domestic relation. If, on the other hand, it were possible to find a district where the general death rate is low, while the infantile death rate is much above the average of such places, you would have, notwithstanding the good general sanitary condition of such a population, a culminating instance

of proved unfaithfulness, on the part of the parents, to their trust; proved neglect, on a large scale, of the duties of domestic life and the care of a family.

In the attempt to state some of the laws of infant mortality I have chosen the period under one year, in preference to the more usual one under five years, as representing the special death rate of the earliest period of life. I have been led to this choice by various considerations which will appear more fully in the sequel, but chiefly by the idea that the utter helplessness of infancy, and its entire dependence upon maternal care, are more fully shown forth at this period than at any subsequent one; and, further, it is evident that the high amount of this death rate, and the great extent of its variations, present facilities for working out results on a small scale, which are not to be obtained in any other way than by taking the very earliest period of life.

Keeping in view then, for the present, only the first year of life, as representing in the most distinct form the hazards to which infant lives are exposed, can we arrive at any secure conclusions as to the relation which the infantile bears to the general death rate? In order to solve this problem I have availed myself very fully of the laborious calculations appended by the Registrar-general for England to his Ninth Annual Report; in which we find a death rate for each sex, and for every separate age of human life in every division and county, and in 324 districts or groups of districts in England and Wales, calculated for the years 1838-44, and for the census of 1841. It is to be regretted that no similarly minute and elaborate series of calculations has been made for any later period; but it will not be difficult for any one who wishes to make a particular application of the principles which may be arrived at in this paper to procure the necessary data for his inquiry, in the case of any localities in which he may be specially interested.*

Every one probably knows generally what is meant by a death rate in modern statistical language. In case there should be any whose memory or knowledge may be deficient on the subject, I will simply say at the outset, that a death rate is the statement of the number of persons of a given population that die annually out of a given number living. If out of a thousand persons living, at all ages, twenty die annually, then twenty is said to be the death rate per thousand for all ages in that

* The census of 1851, though by far the most complete and elaborate that has ever been taken in any country, was, unfortunately, not published in a form to give ready access to the numbers of the population under one year of age; and a large number of important data in the mortality tables have thus been rendered comparatively useless. I have reason to believe, however, that steps have been taken to remedy this important deficiency in the census of 1861.

population. If out of a thousand infants living, below one year of age, one hundred and fifty were to die annually, then one hundred and fifty would be the death rate per thousand of infants in the first year of life. And so of males and females, labourers, servants, farmers, clergymen, noblemen; each class or order into which a population can by possibility be divided so as to ascertain the proportion of its mortality to the whole living members of the class, may be said to furnish forth a death rate of its own. We have to do in the present instance, however, only with two classes, viz., infants of less than one year old, and all ages; the two sexes being, for the sake of simplicity in the statement of results, not distinguished.

Now, on considering broadly the death rate of very young infants, as compared with that of the general population, it is found to be immensely different; insomuch that it is rather a moderate statement of the case to say, that where twenty represents the general death rate, one hundred and fifty will be the death rate of infants less than a year old; or, in other words, that the infantile is seven and a half times the general death rate. This is undoubtedly the case so frequently that it may be said to be, in one sense of the word, a normal fact.

But it is not always a normal fact for the infantile death rate to be seven and a half times the general death rate. By a further consideration of the returns of the Registrar-general, and by a calculation from them, in a great number of instances, of the proportion which the infantile bears to the general death rate, it appears that as these death rates themselves rise or fall, their proportion to one another commonly rises or falls also. Thus, when the general death rate is so low as sixteen in a thousand, it is probably normal for the infantile death rate not to exceed six and a half times the general death rate; and when the general death rate is so high as twenty-two in a thousand, it is probably so common as to be a normal, though, of course, not a desirable result, for the infantile death rate to be eight and a half times the general death rate. Thus it appears, upon a careful and extended consideration of the details of the inquiry, that the proportion or ratio of the two death rates to one another varies with the amount of the death rates themselves. And this curious fact tells a tale of some importance as regards the tenure, so to speak, of infant life—the conditions on which the young infant lives, moves, and has its being; for the enlargement of the ratio between the infantile and general death rate, according as the rates themselves increase, shows nothing less than this—that, generally speaking, the causes which produce a high rate of general mortality have a still greater tendency to produce a high rate of infant mortality, and operate upon the infant life to

a far greater degree. In other words, the infant life is not only more largely sacrificed than the general life of a population under ordinary circumstances, but it is far more keenly sensitive to those causes of increased mortality which produce exceptionally high death rates.

The inquiry thus becomes rather complex. If the case on which I have proceeded be at all correctly stated, it will be necessary, for practical purposes, to determine not only the laws that regulate the rise or fall of the infantile and general death rates, but also the laws that regulate their relation to one another throughout that rise and fall, the limits to which they may diverge and converge, and the conditions under which they may be expected to maintain certain ratios one to the other. And this determination of laws becomes, of course, more difficult in proportion as the varieties in individual cases are more considerable; requiring, under these circumstances, the examination and comparison of a very large number of individual instances.

Suppose for instance that it should be discovered that in one place having a general death rate of twenty in one thousand, only one hundred and twenty infants in one thousand perish annually, while in another place having a like general death rate of twenty in one thousand, the infantile death rate is one hundred and eighty infants in one thousand, it would very clearly follow from this discovery that in the latter place there are certain influences at work specially destructive of infant life. But supposing the general death rate to be dissimilar in the two places, it will then be impossible to compare their infantile death rate with advantage until it has been shown how far the difference of the general death rate may be expected, normally, to influence the infantile death rate of each; in other words, how far the deaths of infants in each place are caused by circumstances acting chiefly upon them, and how far they are due to the pressure of causes of death on the whole population. It is quite conceivable, for instance, that a death rate of one hundred and fifty in one thousand for infants in one place might be more significant of special neglect of infants than a death rate of one hundred and sixty, one hundred and seventy, or even one hundred and eighty in another. Or, to put the case in another form, it might be more significant of special forms of avoidable evil, as regards the management of infants, for the infantile death rate to be seven times the general death rate in one place of very low general mortality, than it would be in another place less favourably situated, were the infantile death rate eight or even nine times the general death rate. It seems, indeed, impossible to develop the full results of the comparison of infantile and general death rates, until an investigation has been made on a

very large scale, and extending over a very large surface, into the normal relations existing between the two rates, and the limits of variation of the ratio between the one and the other.

This inquiry I have endeavoured to carry out—1st, by determining the ratio of the infantile to the general death rate in all the divisions and counties of England, and in a large number of the individual districts and groups of districts; 2nd, by placing these in series according to certain predetermined rules of general arrangement, and reducing them to mean values; 3rd, by arranging these mean values in the form of a scale or table, to be used as a guide in the rest of the inquiry before us.

This table, the result of no small labour and calculation, is a remarkable illustration of the importance of large numbers, and of the collation of many individual facts, in reducing to order a chaos of apparently random variations. It appears quite clearly indeed, from the table, that each unit, and even half unit (within certain limits), in the scale of the general death rate of the populations of England and Wales is normally related to a certain mean infantile death rate, and a certain mean ratio of the one to the other. With the help of this table a new light is shed upon the subject of infantile death rates; and although some of the results may appear to be founded upon too narrow a basis of data, yet their regularly progressive and serial character inspires confidence in them as a whole, and shows clearly that they are in truth the expression of a general law.

TABLE I.

Mean Death Rates (Infantile and General) of Divisions, Counties, and Districts of England and Wales, for the Seven Years 1838-44; showing the Mean Proportion of the Infantile to the General Death Rate, as modified by Variations in the latter Rate. Showing also the very lowest and the very highest Death Rates in England and Wales, together with the very lowest and the very highest ratios of the Infantile to the General Death Rate.

Approximate General Death Rate, for Easy Reference.	Death Rate for all ages per 1000. (500 M.) (500 F.)	Death Rate for Infants under 1 year. per 100. (50 M.) (50 F.)	Ratio of General to Infantile Death Rate.
Lowest death rates (infantile and general) in England and Wales (Glendale),	14.00	7.702	As 1 to 5.50
Lowest ratio of infantile to general death rate (Aberystwith),	17.01	9.091	5.34
Similar ratio, with higher death rates (St. Germain's),	18.59	9.930	5.34
Mean of 9 districts, having very low death rates,	16.24	9.364	5.76
Mean of 22 districts, having general death rates from 14 to 20 per 1000, with relatively low infantile death rates,	17.34	10.364	5.97

TABLE I.—*continued.*

Approximate General Death Rate, for Easy Reference.		Death Rate for all ages per 1000 (200 M.) (200 F.)	Death Rate for Infants under 1 year, per 1000 (20 M.) (20 F.)	Ratio of General to Infantile Death Rate.
	Mean of 2 healthiest counties (Westmoreland, North Wales)*	18.91	11.884	As 1 to 6.29
	Mean of 3 healthiest divisions (S.E., S.W., Welsh D.),	19.41	13.953	7.19
† 16	Mean of all the districts having a general death rate below 17 in 1000,	16.22	10.604	6.49
17.5	Mean of all the districts having a general death rate equal to 17 to 18 in 1000.	17.38	11.407	6.86
18	Mean of 3 counties (Surrey, Sussex, N. Wales),	18.26	12.839	7.03
19	Mean of 5 counties and divisions (North Riding, Dorsetshire, Devonshire, Rutlandshire, S.E.),	19.02	13.730	7.22
19.5	Mean of 11 counties and divisions (Kent, Suffolk, Hants, Cornwall, South Wales, Hertfordshire, Middlesex, Lincolnshire, Westmoreland, Welsh division, S. W.),	19.53	14.788	7.55
20	Mean of 4 counties and divisions (Berks, Essex, Cumberland, E.),	20.02	15.375	7.67
20.5	Mean of 6 counties and divisions (Wilts, Somerset, Shropshire, Norfolk, Northumberland, N.M.),	20.49	16.240	7.91
21	Mean of 11 counties and divisions (Oxfordshire, Bucks, Huntingdonshire, Northamptonshire, Bedfordshire, Herefordshire, Notts, Worcestershire, Derbyshire, S.M., N.),	21.17	17.840	8.44
22	Mean of 11 counties and divisions, including England and Wales (Gloucestershire, Leicestershire, Monmouthshire, Staffordshire, Cambridgeshire, East Riding, West Riding, Durham, W. M., York Division, England and Wales),	22.09	18.798	8.51
25	Mean of 5 counties and divisions, including London (Cheshire, Warwickshire, Lancashire, N.W., London),	24.84	21.507	8.65
27	Mean of 21 great town and city districts (excluding London), which had a population from 50,000 upwards in 1841.	26.83	24.855	9.10

* For purposes of registration, North and South Wales are regarded as counties, and form, along with Monmouthshire, the Welsh Division of England and Wales. In like manner, the Ridings of Yorkshire are held to be counties, and Yorkshire itself a division.

† Up to this point, the figures being deduced from selected instances, cannot be used, without due limitations, as a guide in the interpretation of particular cases. But the mean results, indicated by Egyptian figures, are probably true as approximate averages for all England and Wales (excluding London), at the particular death rate indicated; e.g. when the general death rate is 16, or near it, the infantile death rate is average when it is about $6\frac{1}{2}$ times as great; or, when the general death rate is near 18, the infantile death rate is average when it is about 7 times as great, and so on in increasing proportion.

TABLE I.—*continued.*

Approximate General Death Rate, for Easy Reference.		Death Rate for all ages per 1000 (500 M.) (500 F.)	Death Rate for Infants under 1 year. per 1000 (50 M.) (50 F.)	Ratio of General to Infantile Death Rate.
20	Mean of 14 town and city districts, which had excessively high death rates in the 7 years 1838-44,	28.99	25.858	As 1 to 9.20
	Liverpool (absolutely highest general death- rate),	33.66	28.005	8.82
	Ashton-under-Lyne, including Oldham (absolutely highest infantile death rate, and highest ratio),	26.74	29.641	11.08
	London (absolutely greatest aggregate of city population),*	25.22	20.662	8.19
	England and Wales (general average, inclusive of London),*	21.87	17.975	8.22

I must now endeavour, as shortly as possible (and it must necessarily be so shortly as to amount to a mere statement of results), to bring before you one or two illustrations of the laws of infant mortality, as investigated with the aid of this table. Let me premise that the investigations to which I refer are by no means so complete as is desirable; and in selecting from the mass of notes in my hands I have been guided more by the consideration that the time allowed me is short, than by the idea of doing justice to even one subject. Still, I shall be able, I trust, to show that the method followed is one capable of yielding important truths to a careful inquirer.

First, as regards *the districts and counties having very low death rates both infantile and general, and consequently a low ratio of the one to the other.*† These privileged districts are found to be

* It will be observed that the average ratio of the infantile to the general death rate is much lower in London than is indicated in the table as corresponding to a general death rate of twenty-five; and, accordingly, the average for England and Wales, as a whole, being largely biassed by London, is likewise lower than the mean result corresponding to the same general death rate, which is deduced from the counties and divisions of England and Wales, exclusive of London.

† The following is an enumeration of these districts—1st, Nine districts or groups of districts, having a general death rate below or little exceeding 17 per 1000, while the infantile death rate is less than 10 per 1000—Glendale, &c., Aberystwith, Anglesea, Llanfyllin, &c., Ulverstone, Bideford, &c., Southmolton, &c., Dolgelly, &c., Tregaron, &c.; 2nd, Districts or groups of districts, in excess of the above, but very favourably situated as compared with the country at large—Godstone, &c., Hendon and Barnet, Morpeth, &c., Brompton, &c., Steyning, &c., Havant, &c., Williton, &c., East Grinstead, &c., Swansea, South Stoneham, &c., Totness, &c., Isle of Wight, Narberth and Pembroke, &c., Bangor, &c., Launceston, &c., Bodmin, &c., Guildford, &c., Thanet, &c., Blything, &c., Northleach, &c., Hailsham, &c., Bootle, &c., Tavistock, &c., West Hamp-

mostly rural, often to a great extent pastoral, in character; the population is commonly sparse, the towns few and small, the face of the country uneven, or perhaps mountainous; in many such districts there are valuable minerals and a considerable amount of mining industry; agriculture is also pursued in some of them to a very considerable extent; in others, there is hardly any predominating industry, but a small population supported in a variety of ways upon a soil which does not repay large advances of capital, and which is therefore cultivated by small farmers who give their own labour and that of their servants, but do not employ any considerable number of hands in out-door agricultural employment. Of the districts falling within the description of very healthy districts, the majority are in Wales, Cornwall, and Devonshire; a few of them are in Northumberland, one in Lancashire, one in Salop, one or two in Hants, Surrey, Cumberland, and Westmoreland; the island of Anglesea and the Isle of Wight belong to this order of districts. The largest town found in this connection is Swansea, which had in 1841 a population of 24,604. It is worthy of remark that in Swansea, by far the healthiest town of its size in England and Wales, there is a considerable seafaring, and also a considerable mining population. In Aberystwith (North Wales), a much smaller town, but distinguished by having, next to Glendale, the lowest of all the infantile death rates in England, a very large proportion of the male population is engaged in lead mining. In Liskeard (Cornwall), and in Anglesea, copper mining is largely followed; in Ulverstone (Lancashire), iron and coal are procured in abundance; in Glendale, Bellingham, and Haltwhistle (Northumberland), coal mining is a large branch of industry. Notwithstanding the apparently unfavourable influence, therefore, which some kinds of mining (as Dr. Greenhow has conclusively shown) exert on the health of the men actually engaged in them, it would seem certain, from the experience of these places, that even lead and copper mining are capable of being so followed as not materially to increase the gross mortality of the district in which they prevail; while the death rate of infants is certainly much less unfavourably affected by mining than by many other forms of industry.

Among places which must be pronounced on the whole very healthy, there are some in which the infantile death rate is decidedly greater than, under the circumstances, it ought to be. Such are the districts of Hendon and Barnet in Middlesex;

nett, &c., Haverfordwest, Castleward, &c., Alresford, &c., Cardigan, &c., St. Germans, &c., Atoham, Newtown (Salop), &c., Amesbury, &c. From the latter series I have selected thirteen, which, with the nine in the first series, are referred to in Table I. as illustrations of a relatively small infantile mortality.

Blything, Wangford, and Mutford, in Suffolk; Hailsham, Eastbourne, and Lewes, in Sussex; Thanet and Eastry in Kent; Haverfordwest (including Milford Haven) in South Wales, as compared with Narberth and Pembroke, the adjacent district. The causes of these relatively high infantile death rates would be an inquiry of great interest; but I must not attempt it at present.

Secondly—Among *the districts having moderately, but not extremely low death rates*, a considerable number have characters closely resembling those already narrated. Agriculture assumes a more considerable place as a staple industry; the farms are larger, the proportion of labourers employed is greater; gardening often appears on a great scale, as in the neighbourhood of London; the population is more dense, and the towns more numerous—but still not generally above the rank of market towns of from 4000 to 10,000 inhabitants; there is altogether greater activity, and a greater variety of produce, but usually no absorbing or exclusive industry, manufacturing or other. A large proportion of the districts in the south-eastern and south-western counties of England are in this position; some also in the northern counties, and in Yorkshire; together with most of the districts of Salop, Rutland, and Herefordshire, and some parts of Middlesex.

And here I must notice, in some detail, a circumstance which pertains to this part of my inquiry, and which is full of interest, though of painful interest, for the social reformer. Most of the great corn-growing districts of England belong to the class having a lower than average death rate for all ages; in other words, they have usually been ranked among the healthier parts of the country. Dr. Headlam Greenhow, indeed, threw out some ominous notes of warning about some of them, in his well-known "Report on the Sanitary State of England and Wales;" but as regards others, he appears to acquiesce in the general opinion (current, at all events, since the time when Virgil wrote the *Georgics*), that agriculture on the large scale is one of the wholesomest and happiest of all pursuits; and that a peasantry employed chiefly in the cultivation of the soil is subject to fewer causes of degeneration, moral and physical, than most other classes of men. I am not in a position to prove, and I should be very sorry to suppose, that this opinion is altogether unfounded; indeed, it is certain, from the case of Glendale and many other districts of England, that agriculture, as a leading occupation of the population, is quite consistent with very low death rates, and indeed with the best position in the sanitary scale. I think it quite clear also, from the death rate of all ages in most of the agricultural counties, that to the adult man actually engaged in field work, his occupation is not without some of the bracing and

tonic influence ascribed to it. But I have been greatly startled to find in the great corn-growing counties of England evidences of a flaw in the well-being of the infant population, which must necessarily exert a deleterious influence on the health of these counties, and through them on the English race in general. Not only is the infantile death rate in many of them high (absolutely higher, for instance, than that of the country at large), but in many cases where this is not so, the infantile death rate is much higher than it ought to be, considering the amount of the general death rate; and higher also than it ought to be, considering the eminently rural character of the population, the small size of the towns, and the small number of persons to each acre of surface. These facts are illustrated in Table II.

I have not adopted this conclusion without carefully revolving the matter, and subjecting it to analysis from a great many different points of view. But this much is certain, that in no less than eleven of the fourteen counties of England most devoted to agricultural pursuits (*i.e.* employing the greatest number of agricultural labourers) the ratio of the infantile to the general death rate is higher than that indicated in the table as the mean ratio for their general death rate; and, further, in some of them the ratio is enormously high, exceeding in fact that assigned in the table to the fourteen worst town districts, and to the counties and divisions of England having absolutely the highest death rates, both of infants and of all ages. Again, in five of these fourteen counties the infantile death rate is higher than that of England and Wales, although England and Wales have 307 persons living on each square mile, while the agricultural counties have only (on a mean estimate) 224.

It is difficult to give within reasonable limits an idea of the precise course by which I have arrived at the conclusion, that in almost all of the eminently agricultural counties the destruction of infant life is in excess of what might be expected under the circumstances. I will, therefore, only state, as the general result of my inquiries, that the evil is least, indeed hardly apparent, in Wiltshire, Berks, and Herefordshire; that in Essex, Suffolk, Bucks, and Oxfordshire, it is unequivocally present, and to a still greater degree in Hertfordshire. In Northamptonshire the high infantile rate also exists, but may be due in part to the towns of Northampton and Peterborough, which have a character quite distinct from the rural population. In Huntingdonshire, Cambridgeshire, Bedfordshire, Lincolnshire, and Norfolk, the infantile death rate reaches its maximum, being considerably above that of England and Wales; and the proportion between the infantile and general death rate is more than 1 in 9—being in Norfolk 1 in 9·38, and in Lincoln 1 in 9·36. The case of

Infantile and General Death Rates (1888-43) in 14 Counties, in which more than 30 per cent. of the Male Population above 20 years of age are employed as Agricultural Labourers. Showing also the Proportion of the Infantile to the General Death Rate, the Proportion of the Population living in Towns, the Density of the Population, &c.

TABLE II.

Agricultural Labourers in each 100 Males above 20 years.	Persons to One Square Mile.	Per cent. of Population in Towns.	Name of County.	Name of Division.	Death Rate for all ages per 1000. (60 M., 60 F.)	Death Rate for Infants under one year per 1000. (60 M., 60 F.)	Ratio of General to Infantile Death Rate.	Special Industrial Occupations other than Agriculture.* Principal Towns.
40.2	186	25	Huntingdonshire, .	S.M.	21.42	19.296	As 100	No special industry. Towns small.
40.2	272	30	Bedfordshire, .	S.M.	21.22	19.806	9.00	Lace, Strawplait, &c. Bedford, Luton.
87.7	231	29	Suffolk, .	E.	19.67	15.619	9.38	Silk, Strawplait, &c. Ipswich, Bury.
87.1	224	26	Essex, .	E.	20.19	16.611	7.89	Silk, Strawplait, Shipping. Colchester.
86.4	216	31	Cambridgeshire, .	S.M.	22.41	20.821	9.07	No special industry. Cambridge, Wisbeach.
86.1	260	25	Hertfordshire, .	S.M.	19.74	17.685	8.95	Strawplait, &c. Towns small.
86.1	198	45	Wiltshire, .	S.W.	20.29	14.188	6.96	Wool, Silk, &c. Salisbury, Towbridge.
86.0	228	37	Buckinghamshire, .	S.M.	21.32	17.426	8.17	Lace, Strawplait, &c. Aylesbury.
84.8	149	25	Herefordshire, .	W.M.	21.14	14.988	7.09	Gloves. Hereford.
88.6	227	32	Oxfordshire, .	S.M.	16.901	16.901	8.07	Lace, Gloves, &c. Oxford, Woodstock.
82.7	226	80	Berkshire, .	S.E.	19.84	14.179	7.15	No special industry. Reading.
82.6	216	28	Northamptonshire, .	S.M.	21.07	17.687	8.89	Lace, Boots and Shoes. Northampton, Peterborough.
81.6	213	86	Norfolk, .	E.	20.06	19.881	9.38	Silk, &c., Shipping. Norwich, Yarmouth, Lynn.
80.8	147	39	Lincolnshire, .	N.M.	19.88	18.147	9.86	No special industry. Lincoln, Boston, Great Grimsby, Grantham, Louth.
85.4	224	31	Mean of 14 Counties,	...	20.66	17.218	8.82	
15.4	307	50	England and Wales,	21.87	17.976	8.22	

* Only those industrial occupations are inserted which are so prevalent as possibly to have some appreciable influence on the death rates of the country as a whole.

Lincolnshire is peculiarly striking; for the general death rate is only 19·38 per 1000; while the infantile death rate is more than 18 per cent., instead of 14 per cent., which is the normal amount in the circumstances, according to the table. Moreover, Lincolnshire has no considerable and generally diffused industry, with the exception of agriculture; the density of its population is remarkably small, being only 147 persons in one square mile (not much greater than the density of Wales, and much less than that of Cornwall); it has few large towns to enhance the death rates, and the largest of its town districts has an average, as regards infantile mortality, considerably below that of the whole county.

These circumstances, in regard to Lincolnshire, lead me to make a remark upon the causation of the high infantile death rates in agricultural counties, or rather, upon the difficulties in the way of arriving at a just conclusion in regard to that subject. On the one hand we have, I think, distinct evidence that a large surface of soil, devoted almost exclusively to agriculture, is often associated, in England and Wales, with a too high rate of infantile mortality. On the other hand, I think it not less evident that agriculture, *per se*, is not the determining cause of the mischief; the district of Glendale alone being enough to prove that it is possible for a large proportion of population to be engaged in agriculture without any unfavourable effect upon the death rate. What then is the solution of this two-sided difficulty—first, agriculture, an occupation apparently eminently favourable to low death rates, in the case of Glendale and other districts; secondly, agriculture, where largely diffused as an industry over the Midland counties, apparently unfavourable to infantile life?

Dr. Headlam Greenhow, in the remarkable State paper to which I have formerly alluded, has insisted strongly on the danger frequently accruing to the health of the female population, and of the children, in rural districts, from the occupations of lace making, straw-plait weaving, straw-bonnet making, &c.* The tendency of his remarks, in some parts of his inquiry, would indeed appear to be in a direction equally consistent with sound morality and correct principles of social economy—viz., towards establishing the principle that nature intends woman to be, first of all, a mother; and in no instance, on the great scale, to be anything inconsistent with this, her first and indispensable function. That a law so clearly written in the whole physical constitution and moral nature of woman should not be largely violated with impunity, is what every thoughtful student of social science must be prepared to believe; and, so far as my

* Papers relating to the Sanitary State of the People of England, &c. 1858. See especially pp. 28, 55-7, 81, 121.

inquiries have gone, I think it is plain that the influence of special industrial occupations among the women of the agricultural districts, is, to say the least, not favourable. But the reader of Dr. Greenhow's remarks, especially at page 122 of his paper, cannot fail to be struck with the uncertainty of his tone, and indeed with the discrepancies among his facts, as bearing on the relation of industrial employments of women in general to infantile mortality. I regret that I am obliged, in the meantime, to maintain a similar reserve in discussing this point, which would require a much larger and more detailed acquaintance with the localities than I possess, to enable me to do justice to it. But, whatever be the practical influence of lace making, straw-plait, or silk manufacture, in the agricultural counties, it is not the sole, nor even the principal cause of infantile mortality. For it is only, at most, in seven of the fourteen agricultural counties that these manufactures acquire any decided preponderance; and in Huntingdonshire and Lincolnshire, which are almost purely agricultural as to special employment, whether of men or women, the death rate of infants is far above the mean, even of the agricultural counties; while in Cambridgeshire, in which there is no special industry of the women, the infantile mortality is absolutely the highest of the agricultural counties, and much higher than the average of England and Wales, or even of London, or indeed of any part of England, with the exception of Lancashire, and the great manufacturing towns.

It is not improbable that the low level and malarious atmosphere of some parts of the agricultural counties, particularly in Lincolnshire and Cambridgeshire, may produce an unfavourable influence on the health of the offspring; but in the former county, where the general death rate is decidedly low, this influence will hardly account for the occurrence of an infant mortality so much out of proportion to the general death rate. Besides, it has yet to be proved that malarious influences, generated in connection with the soil, have a greater power over the young children of a population than over the men, so much more decidedly and constantly exposed to these emanations in the course of their work in the fields.

On the whole, I can arrive at no other conclusion for the present than this—that the habits of the great agricultural populations of England, probably of slow formation, and transmitted down from generation to generation, are, in some way or other, apt to give rise to neglect of the family relation, or of maternal duty, or of the general sanitary laws bearing on the health of offspring; and that the extensive employment of the women in some counties in special industries is one consequence of this habitual neglect; while the imperfect rearing of children is

another, and a still more widely spread consequence of it. It remains for the local social inquirer, and especially for those who are the natural guides and instructors of the agricultural population, to detect more in detail, and to remedy as far as possible, this deplorable evil.

III. *The districts having the highest death rates* present a fruitful subject for remark. I might easily enlarge, to a great extent, upon the terrible sacrifices of infant life that take place annually in the manufacturing districts, in the Potteries of Staffordshire, in many of our seaports, and, generally speaking, in all the worst parts of our great centres of population; in some of which it would appear (unless the details are incorrect) that much more than 1 in 4 of those living under one year of age perish annually. These results, however, though painful and startling, could hardly be placed in a more clear point of view than they have often been placed before. They are, moreover, well known to all sanitary reformers, and have often been brought under the notice of this association. I pass, therefore, to other and less familiar topics.

IV. I have next a few words to say about *the infantile death rates of London*—a subject of vast extent and importance, and well deserving of much more time and attention than can here be given to it at present.

As in the case of many other town districts, the death rates for all ages are, in London, a very imperfect index to the sanitary and social state of the population. But in London there is a very special reason why it is so, particularly as regards the West-end districts. The census is taken during the height of the London "season," when vast numbers of persons, most of whom belong to the active and healthy class, come up to town for business or pleasure, bringing with them also a train of domestic servants and other dependents, who would not be brought at all if they were known to be labouring under disease, or otherwise likely to swell the death rates. By a process of what Mr. Darwin would call "natural selection," therefore, the West-end, during the season, consists, to a great extent, of picked specimens of humanity. Its denizens, moreover, are persons who frequently have their real homes elsewhere; who, if they were to fall out of the ranks through sickness or accident, would not remain in London, but would hide their heads in the country, beyond the bustle and glare of the fashionable world. Against this we may undoubtedly set a certain proportion of sick who come up to London for medical advice, and may die there. But the immensely preponderating result is found to be that London (meaning by London, the West-end) is crowded and packed with health during the "season," when it becomes the great magnet

of attraction for wealth and fashion, the centre to which all the most active and vigorous of men, and the most beautiful and fascinating of women, gravitate from all parts of the kingdom. The census takes note in spring or summer of this packed but fluctuating population, the feeble and sickly members of which have been to a great extent spontaneously eliminated, and the healthy portion of which is ere long to be dispersed over Scotch moors and Swiss mountains, or sent into pleasant English country houses. Thus, the annual mortality of London, as recorded from week to week by the Registrar-general, by no means represents the mortality of the numbers included in the census; and this remark is peculiarly applicable to what is commonly called the "West-end." It is not surprising, therefore, under these circumstances, that the West-end districts of London should have a general death rate much below the average of town districts—that in St. George's, Hanover Square, only 18 in 1000 should die annually; in St. James, Westminster, 21 in 1000; in Marylebone, under 23 in 1000; and that, even taking into account the "slums" of Westminster, and the comparatively very inferior population of St. Martin-in-the-Fields, the mean death rate of all the districts which are the great seats of business and fashionable life, in the West-end of London, should be decidedly below the average of London as a whole, and still more below the average of other great cities.

Of course there are many large populations in London by no means so favourably placed as regards their general death rates. Westminster, as already mentioned, has its teeming dens of wretchedness which raise it to a rate of nearly 26 in 1000; the crowded districts immediately surrounding the "city" have a still higher rate (the city itself, however, being comparatively healthy); and from this we reach by gradual steps the culminating death rates of 28·46 in 1000 for St. Saviour and St. Olave, 28·87 for St. George-in-the-East, and 29·03 for Whitechapel.

Now we might naturally expect that in these different districts the infantile mortality would bear some appreciable proportion to that of all ages. But the fact is far otherwise, inasmuch that it is absolutely impossible to extract from the death rates of London any trace of such a series of proportions as is shown in Table I. to exist in the counties of England taken as a whole. The proportion, in fact, between the infantile and general death rate seems at first sight so purely anomalous and arbitrary as to defy interpretation. Thus, in the Strand district, with a general death rate of 24 in 1000, the infantile death rate is little more than 19 per cent., while in St. Martin-in-the-Fields it is nearly 24 per cent., and in Kensington and Chelsea upwards of 23 per

cent., with nearly the same general death rate. Again, in Greenwich, with a general death rate close on 23 in 1000, the infantile death rate is as low as 16·5 per cent., while in Marylebone it is as high as 22·9 per cent., for the same general death rate. Or to take, if possible, a more extreme instance of variation, in Hampstead the general death rate is 20 per 1000, and the infantile death rate 14·6 per cent., or about seven times the former; in St. George's, Hanover Square, the general death rate is lower than in Hampstead, being about 18 per 1000; but the infantile rate is more than 20 per cent., being no less than eleven times the general death rate, and between 5 and 6 per cent. more than the infantile death rate of Hampstead.

These facts hardly admit as yet of being reduced to any general form of expression. But a careful consideration of them has led me to the discovery of a phenomenon which lies, indeed, on the surface in the Registrar-general's returns, but which I do not remember to have seen stated in the distinct form in which I shall now bring it under your attention. It is this—that all the West-end districts of London, without exception, are fatal to children, in a proportion which is really enormous when we consider the favourable state of the general death rate, and the many advantages which these districts have over the others. Let us take, for example, St. George's, Hanover Square, which, with the exception of Kensington, has the lowest infantile death rate of all these districts, and which, as we have seen, has very nearly the lowest general death rate of all London. The district of St. George's, Hanover Square, with all its wealth, its splendour, and really English comfort, is only a little less fatal to infants than Shoreditch, Bermondsey, or Lambeth; it is more fatal than the Strand district, or Stepney, which have a general death rate of twenty-four per thousand; far more fatal than Greenwich, the general death rate of which is nearly twenty-three; and in a still more striking proportion more fatal than Wandsworth, Camberwell, and the outlying districts in general.

St. James, Westminster, has an infantile death rate almost precisely corresponding to that of the river district of St. George's-in-the-East, and exceeding that of Poplar, Clerkenwell, and Bethnal Green; Marylebone, tested by the infantile death rate, must submit to be ranked as less healthy than the Surrey river-side districts of St. Saviour and St. Olave, and also than St. Luke's; while St. Martin-in-the-Fields actually exceeds Whitechapel, and approaches St. George's, Southwark, in its infantile mortality.

TABLE III.

Mean Death Rates (Infantile and General) of various Groups of Registration Districts in London (1838-44). Also, Death Rates of several Single Districts, and of all London.

	Death Rate for all Ages, per 1000. (800 M.) (800 F.)	Death Rate for Infants under one year per 100. (50 M.) (50 F.)	Ratio of General to Infantile Death Rate.
A. Group of 3 districts adjoining the country, on the south side (Wandsworth, Camberwell, Lewisham)	18.90	15.140	As 1 to 8.01
B. Group of 4 districts adjoining the country, on the north side (Hampstead, Pancras, Islington, Hackney)	20.52	16.916	8.24
C. Group of 5 West-end districts not adjoining the country (St. George, Hanover Square; St. James, Westminster; Westminster, Marylebone, St. Martin-in-the-Fields)	22.40	22.086	9.96
D. Group of 4 districts, in the line from Charing Cross to Bank and Leadenhall Street (Strand, City, E. and W. London)	24.09	20.806	8.65
E. Group of 3 districts adjoining the river on the north side, and including the docks, &c., (St. George-in-the-East, Stepney, Poplar)	25.70	20.339	7.94
F. Group of 3 East-end districts, adjoining (D) and (E) (Whitechapel, Shoreditch, Bethnal Green)	25.99	22.097	8.53
G. Group of 4 districts adjoining (D) on the north (St. Giles, Holborn, Clerkenwell, St. Luke)	26.31	24.166	9.18
H. Group of 4 districts adjoining the river on the south side (St. Saviour, St. Olave, Bermondsey, Rotherhithe)	27.51	21.564	7.84
I. Group of all the remaining districts of London (Greenwich; St. George, Southwark; Newington, Lambeth, Kensington, and Chelsea)	28.87	20.421	8.47
a. Lewisham (Group A),	17.26	13.980	8.07
b. Hampstead (B),	20.24	14.618	7.22
c. St. George, Hanover Square (C),	18.16	20.245	11.15
d. St. James, Westminster (C),	21.16	21.756	10.28
e. Marylebone (C),	22.72	22.937	10.09
f. St. Martin-in-the-Fields (C),	23.99	23.868	9.94
g. Strand (D),	24.17	19.353	8.01
h. Shoreditch (F),	25.07	20.990	8.87
i. Bermondsey (H)	26.38	20.723	7.85
k. St. George-in-the-East (E),	28.87	21.721	7.52
m. Whitechapel (F)	29.03	23.575	8.12
n. St. George, Southwark (I)	26.68	24.388	9.12
o. St. Giles (G),	26.89	28.249	10.50
All London,	25.22	20.662	8.19

In the table of the death rates of London hereto annexed (Table III.), I have arranged the districts in groups, for each of which a mean has been calculated. The arrangement of districts is partly geographical, and partly founded on other considerations which will be sufficiently evident on inspection. It results from this table, that the group of districts which I have marked on this map* has a position inferior to all the others except two; the two in question being simply a collection of the most crowded, and among the most neglected districts of London. In other words, the group of districts which incloses all that is best and noblest and, in one sense, healthiest and most vigorous in London, is about as murderous to infants under a year old as the districts of Shoreditch, Bethnal Green, and Whitechapel, taken together; while even the sailors around the docks, and on the Surrey shore, and the tradesmen and artisans of the Strand and City districts, may boast that their contribution to the infant mortality of London is small compared with that of the rich, prosperous, and polished West-end.

I have thus brought before your notice a few illustrations of the ends to be gained by investigating infantile death rates. Some of the deductions from my inquiry are no doubt familiar enough to all who have given the least attention to the subject. Two of my conclusions—one, the influence of large agricultural populations on the infantile death rate; the other, the influence of the West-end of London—have, I confess, startled me with something like a sense of novelty. If they be facts, and not mere play of the fancy, I venture to call them stupendous facts, deserving the immediate attention of this Association, and calling for remedy with a voice far more eloquent than any words in which they could possibly be uttered. I shrink from the idea, with the imperfect data before me, of attempting to expatiate upon this subject, or to explore the whole depths of these tremendous evils. Possibly, if the Association will permit me to address them on a future occasion, the subject may be again resumed with larger knowledge and in a more decisive tone. In the meantime, it will be sufficient for my purpose if the attention of this Association, and of the numerous members of it who have personal opportunities of inquiring into the matter, shall have been fully awakened to some of the evils which afflict the most helpless age of humanity, and tend to poison the springs of health for the entire English race.

* St. George, Hanover Square; St. James, Westminster; Westminster, Marylebone, St. Martin-in-the-Fields.

III. *Cases of Purulent Deposit in the Urine.* By ALEX. LINDSAY, M.D., Fellow of the Faculty of Physicians and Surgeons, &c.

Case I.—Large Deposit of Pus in Urine, following Gonorrhœa—No Symptoms of Local Irritation—No Constitutional Disturbance.

June, 1849.—J. L., a healthy man, aged 29, house-wright, consulted me in reference to some unusual appearance of his urine. He complained of no local uneasiness. His general health was excellent, the appetite being good. Bowels regular, and he felt quite able for his employment. His appearance and statements led me to conclude that this was one of those suppositious cases of disease so often met with associated with the urino-genital organs. To ascertain the truth, he was requested to furnish me with several ounces of urine for examination, and the usual instructions were given in order to obtain an average specimen. On the following day four ounces were received.

The urine was turbid and opaque, with a faintly disagreeable odour, and was slightly alkaline. On standing some hours a dense yellowish cream-like deposit separated. This occupied more than the lower third of the vessel. The supernatant portion had the usual appearance of the healthy secretion. On boiling a portion of this latter and adding nitric acid, it was seen to be markedly albuminous. The deposit presented all the characteristics of pus, and examination by the microscope showed conclusively that this was in fact the morbid product.

It was elicited on further enquiry that four months before, he had contracted a gonorrhœa, and that the discharge had apparently disappeared after some three or four weeks' treatment. He was not aware of the nature of the medicine that had been prescribed. He had continued, so far as he knew, perfectly well till about six weeks before he consulted me, when the urine had begun to acquire its present appearance. Gradually it seemed to be getting worse up till the two last weeks, since which time it appeared to have varied but little. Again, a most careful examination furnished no evidence of general disorder, nor, as before stated, of local irritation. After clearing out the bowels, he was ordered to have one drachm of the bals. copaiva in mixture, night and morning after meals, to avoid all stimulating drinks, and to restrict himself to light diet. The effect of the medicine was most apparent. At the end of three days after the first administration, the turbidity of the urine had nearly gone and, after eight days, it had completely disappeared, no pus being visible. He was recommended to continue the use of the balsam for some days to prevent a recurrence of the symptoms.

In January of last year this man died. Within eighteen months subsequent to being under treatment he married. Ultimately he fell into very irregular habits, and suffered from frequent exposure. Up to the time of his death, which followed some acute affection of the chest, he never complained of any urinary annoyance.

Case II.—Purulent Deposit in the Urine Supervening on Gonorrhœa—Severe Local Symptoms.

December, 1852.—Late in the evening I received an urgent request to visit a pupil, who was stated to be suffering from a very severe and painful illness. He had attended the class as usual on the previous evening, and had then no complaint. On reaching his lodgings a few minutes after I received the message, I found him in great agony. He complained of distressing pain in the hypogastrium and perineum, which pressure did not increase. There was a constant desire to micturate, the urine coming away in very small quantities. The countenance was anxious; pulse small and rapid; tongue dry. The bowels were stated to be regular. These symptoms had only set in since the morning, and had assumed their present violence within a few hours previous to my visit, although I now learned that he had been rather ailing for two days. Previous to my seeing him, six leeches had been applied to the perineum, some fellow-students having concluded that the case was one of inflammation of the prostate. He denied having had gonorrhœa, although I had my suspicions on this point.

The leeches acted well, and on their removal, thirty drops of laudanum were administered. This was followed by an enema of castor-oil and thin gruel, and some hours after the action of this, unless there was a marked and decided improvement, he was to have another enema of laudanum and starch.

When visited next morning, he expressed himself as much relieved, and had slept a little. He, however, dreads a return of the sufferings of the previous evening. The urine passed had been retained for examination. It was opaque and muddy. On being allowed to stand, a copious deposit settled down, which was found to be pus. Admitted now having had a gonorrhœa, which he had felt a delicacy in confessing before his companions. This he had supposed to have been well for some weeks. He had at first used copaiva, and latterly mild injections. He was ordered to have night and morning a mixture containing the balsam of copaiva in small doses, gradually to be increased or diminished as the symptoms indicated.

Rapid, almost immediate improvement was the result. Under

the use of the remedy all the annoying symptoms gradually disappeared, the urine assuming its ordinary character. This gentleman has long since married, and the symptoms have never shown any tendency to recur.

Case III.—Severe Hypogastric pain, following a sudden Suppression of a Gonorrhœal discharge—Retention of Urine—Pus in Urine.

June 1st, 1856.—J. B., wright, had contracted a gonorrhœa three weeks before consulting me. He was at that time treated by an irregular practitioner, who supplied him, with a mixture that acted freely on his bowels and occasioned considerable sickness, but stopped the running in three days. A week after the discharge ceased, he began to complain of uneasiness at lower part of abdomen, with a more frequent desire than usual to empty the bladder. The symptoms increased up to the time that I saw him, when the pain had become very severe. He was able, however, to continue at his work, although weak and feverish, with restlessness at night; appetite much impaired; bowels irregular. Was ordered half an ounce of castor-oil with four grains of calomel, to be taken at bedtime; and to have a teaspoonful three times a day of a mixture consisting of equal parts of the spirit of nitric ether, liquor potassæ, and tincture of hyoscyamus, and to use diluents freely.

5th.—No improvement. Thought he was rather worse. He was ordered to continue the mixture, and to have a rhubarb pill every alternate night.

11th.—On the evening of this day I was requested to visit him and found him to be in very great distress. No urine had been passed for twelve hours. The abdomen was much distended, and the features anxious. Bowels not opened for two days. A catheter was passed with perfect ease into the bladder, when a large quantity of urine escaped. This was followed by immediate relief. He was to have an enema of warm water, fomentation to lower part of belly, and thirty drops of laudanum after the operation of the injection.

14th.—To-day I saw him in consultation with Mr. Fergus. His friends had become anxious, and a troublesome cough had set in which was annoying him. No improvement had taken place in his other symptoms, the use of the catheter being daily required. A small blister was ordered to the chest, and an expectorant mixture was prescribed, to be continued till the state of the urine was ascertained—this having been hitherto quite overlooked.

15th.—Much as at yesterday's visit. To my surprise the urine, set aside for investigation, presented quite a similar

appearance to what it did in the cases already detailed, there being as in them a remarkable freedom from every other morbid product, except pus—examination by the microscope making this evident.

With the concurrence of Mr. Fergus he was ordered copaiva in half-drachm doses night and morning. Within twenty-four hours after commencing the medicine he was able to void his urine without instrumental aid, and daily continued to improve, the purulent deposit gradually disappearing. Although the relief to the local symptoms was, as we have seen, almost immediate, yet, as regards his general health, the convalescence was slow. For a considerable time he was weakly, so much so that a residence for a few weeks in the country was required. Ultimately he regained his strength, and remained well for at least two years, when he left Glasgow.

Case IV.

It is needless to enter into all the particulars connected with the progress of this case. In many respects it closely resembled the first; and there were neither marked constitutional nor local symptoms. The treatment, however, was much more protracted than in any of the others, for which no very obvious reason could be assigned. I had him under observation from the first. He sought advice for a urethral discharge of some eight days' standing. This slowly yielded to treatment, but immediately on the cessation of the gonorrhœa, the pus in the urine appeared. Further administration of the balsam had for a time to be suspended, and occasional laxatives substituted, with half a drachm of the muriated tincture of iron twice a-day. After a time the copaiva was resumed with a successful issue, the deposit ultimately quite disappearing. Of the after history of this gentleman I have no information, as some months after he left Glasgow.

Case V.—Pain in left iliac region, extending over bladder, and paroxysmal in character—Previous Gonorrhœa—Return of Symptoms.

September 30th, 1857.—Mrs. B., aged 45, tall, and of delicate habit, came under my care with the following symptoms. Acute pain in the right iliac region, extending over lower part of belly. It is not constant, as she occasionally feels quite in her usual way; is able to go about and attend to her household duties. Sometimes she is annoyed with a frequent desire to pass urine, and then the urine feels hot, and its passage painful. No pain on pressure. Two months before seeing me had whites, which had, however, quite disappeared. She had used no treatment

other than washing out the vagina with tepid water. She was recommended to use nourishing food, to attend to the state of the bowels, to avoid over-exertion, and to apply turpentine stupes when the pain was severe.

For three weeks the symptoms remained much as when I first saw her. This continued till the 1st of December, when I was requested to visit her. Then I learned that within a day or two her sufferings had become greatly increased, intense pain recurring at nearly regular intervals of ten minutes, and the severity of the attacks causing her to scream aloud. There was constant desire to micturate, the urine passing in small quantities. Bowels costive; pulse rapid; no appetite, and was losing flesh. Pressure on left side of lower part of abdomen caused no pain, but there was great tenderness over hypogastrium. Examination of the vagina aided in no way in removing the obscurities of the case. She was ordered a teaspoonful of the spirits of nitric ether, with a full dose of laudanum; this to be followed by a castor oil and thin gruel clyster, fomentations to the lower part of belly, and the vagina to be washed out with warm water every three hours. Urine to be retained for examination.

On calling on the following day, I found her much as at first visit. The treatment had afforded no relief. To my surprise, the urine set aside showed a purulent looking deposit, which shaking readily diffused through the upper clear portion of the fluid. The latter was found to be markedly albuminous. Caustic potash produced its effect on the deposit that follows on adding it to pus, and the microscope confirmed the conjecture as to its true nature.

On questioning the husband, which I was now prompted to do, he at once admitted having had a gonorrhœa; that he had contracted the disease about four months before, and that, before he was quite well, he had been in circumstances to communicate it to his wife. He added that the discharge was so trifling that he thought there was little risk.

Being thus guided, I cautiously began administering the balsam of copaiva, beginning with doses of one half drachm twice a day. With this was conjoined ten minims of aromatic spirits of ammonia, and an equal amount of tincture of hyoscyamus. Under this treatment she slowly improved, and in three weeks was free from all annoyance. Some months after, the same symptoms returned. Similar treatment was employed, and they shortly passed away. Up to this date (Feb. 21) there has been no return, and she is otherwise in good health.

In the remarks that are to follow, brevity shall be studied, and thus the time of the reader will be saved, and his patience best considered. The cases, moreover, speak for themselves,

rendering lengthened comment unnecessary. The appended observations are, therefore, not intended to elucidate the details that have preceded, but will rather aim at endeavouring to secure attention to morbid states, alike interesting and important, whether viewed in relation to their origin, their symptoms, or their treatment.

Systematic writers do not seem to include, as incidental to gonorrhœa, complications similar to those described. It is true they may have been overlooked, yet equally certain that among the more recent writers they are not mentioned. This is the more curious, seeing that in my practice they have been met with more frequently than other affections of the urino-genital organs that have received, not only a special notice, but even a lengthened description. It is possible, however, that my experience may have been exceptional.

Pus in the urine is not uncommon ; it may be accidental and temporary, or, on the other hand, it may be persistent. Whatever the circumstances in which it appears, its presence as a symptom must be viewed with suspicion, and, if of long continuance, it is one of grave import. A knowledge of this gives a special interest to the foregoing cases. In them, the admixture of pus with the renal secretion was an important feature. In one individual it was long continued, and its presence the only annoyance. Yet, while this was so, they were readily subject to treatment, the remedy, with one exception, acting speedily, and in all safely. In the practice of others, similar results have followed in two cases. These were like in origin, and allied in symptoms ; and the same success followed the administration of the medicine so often already named.

In reference to the selection of the remedy no merit whatever is claimed. It is not likely it would have been chosen, but for the benefit following its use in the first case. The symptoms, local and constitutional, in the others seemed to contraindicate its employment, and it was only after remembering that they had the same history, and observing the close resemblance of the urinary deposit, that I ventured on its trial, fortunately with a large measure of success. The uniformity of the result is the more surprising when the uncertainty of the action of copaiva in urethral discharges is remembered.

As to the nature of the morbid change on which the symptoms were dependent, I can offer no information. Ingenuity, no doubt, might be displayed in hazarding a guess, but the result would of necessity be unsatisfactory. To obtain in regular sequence the various links that associate morbid phenomena, is always desirable. To reach this is the one great object of scientific medicine. Yet it cannot always be accomplished, and we

are obliged to rest satisfied with founding our treatment on the lessons of experience. Here, however, it may be noticed that some have supposed that prostatic inflammation, or cystitis, limited to the neck of the bladder, would suffice to account for the symptoms. Now, when the gland is inflamed, abscess, and not a constant formation of pus, is the characteristic; and in the cystic complication, mucous is largely mixed up with the deposit in the urine, the former being stringy and tenacious. In all the cases seen by me, there was no perceptible mucous admixture, and no cohesion among the particles composing the deposit. This is important, and requires to be remembered in determining as to treatment in cases having a relation to those described. Pus alone, unmixed with any other inflammatory product, appears to be the diagnostic guide.

IV.—*Contributions to the Pathology and Therapeutics of Typhus Fever.* By JOSEPH BELL, M.D., Physician and Clinical Lecturer, Glasgow Royal Infirmary.

No. VI.

I. AFFECTIONS OF THE BRAIN AND NERVOUS SYSTEM.

II. DISEASES OF THE PULMONARY ORGANS.

I. THE BRAIN AND NERVOUS SYSTEM.—In directing attention to the effects of the typhus poison on the brain and nervous system, no introduction is necessary. That lesions of the cerebro-spinal system are the most frequent cause of death in fever, is a fact too familiar to every practitioner to demand a single remark to be made regarding the great importance of such complications.

The symptoms of lesions of the brain are—1st, Want of sleep; 2d, Subsultus; 3d, Delirium. The supervention of one or more of these phenomena, indicates that either functional or structural disturbance of the brain has commenced. The manifestation of such symptoms merits our most anxious and careful consideration.

The affections of the brain may be classified under the following five varieties:—

I. The most frequent, and at the same time the least serious kind, occurs to a greater or less extent in almost every case of fever. About the tenth or eleventh day, the patient's mind becomes feebler; he appears to dream very often, and occasionally wakes up under the impression that the dream is a reality; he will speak and act for a few minutes under this belief, but soon

perceives the mistake. This state may continue till the crisis, but not unfrequently he becomes worse; he begins to see objects around his bed, imagines that he is away from home, or perhaps engaged at his usual occupation. These delusions not only arise after sleep, but occur whilst he is awake. If, however, his attention be suddenly arrested, he will talk rationally for a short time and then relapse into his imaginary world. About the fourteenth day he will be found lying on his back talking incoherently, or muttering during his slumbers; then gradually falls into a sound sleep, from which he awakens much improved in every respect.

In this class of patients there is not much *want of sleep*, and the *subsultus* is *slight*.

Prognosis in such cases is favourable; the mental perturbation generally subsides, along with the febrile symptoms, about the fourteenth day.

Treatment.—Careful watching is the only treatment demanded; the patient should never be left alone. At any time, under the impression that either his dreams or delusions are realities, he may rise and seriously injure himself. This watching is the more urgently required at the commencement of the raving, because the patient's strength is then considerable, and he is able to leave his bed and walk about; but, at a later period of the attack, he is not able to do so, and hence there is less risk of serious consequences. Therefore, when the patient commences to manifest symptoms of mental alienation, however slight, he should be watched with great vigilance. I have known very serious accidents happen from the neglect of this precaution. For example, a patient dreamed that there was a robber secreted in the chimney; he rose and commenced to clamber up the fire-place in pursuit. He very soon fell from exhaustion, and was discovered in a most deplorable condition—cold, nearly pulseless, and covered with soot, his forehead severely cut from falling against the fire-irons. He died two days afterwards, and I have no hesitation in expressing my belief that the fatal result was to be attributed to the accident—which careful attention would have prevented. I attended a young gentleman, two winters ago, who about the eleventh night of his illness, in the absence of his nurse, rose from his bed under the influence of a dream which he had regarding a friend at the sea-coast; he partially dressed, snatched his carpet-bag, and commenced to unlock the door, when he was fortunately discovered. Had this not taken place, in a few minutes he would have reached the street. I have met with numerous instances of a similar kind, demonstrating the urgent necessity for the strictest watchfulness being observed when failure of the mind is perceived.

I have already mentioned that *want of sleep* is seldom mani-

fested in this class of cases, but when it does appear, I have either given a moderate dose of opium and a few grains of James' powder by the mouth, or a laudanum injection. If subsultus exist to any considerable extent, a few grains of camphor every six or eight hours should be given. When the bowels have been inactive, I have seen much mitigation of both raving and subsultus follow the operation of a mild purgative enema.

II. In the second variety, the symptoms make their appearance about the same time as in the first, and during a day or two exhibit little or no difference, but, about the twelfth or thirteenth day, the patient falls into a slumbering condition, his breathing becomes very peculiar, for a considerable period no inspiration takes place, then a very deep one followed by a few rapid short ones, next a lengthened repose, then the deep inspiration which is again succeeded by a series of short ones, and so on. This kind of breathing has been well described by the Dublin physicians, and denominated by them "*Cerebral*." It indicates the approach of most serious mental disturbance ending in profound coma.

The *prognosis* in such cases is unfavourable. Unless the breathing be observed at its commencement, the case will prove fatal.

Treatment.—The remedies which I have found most useful in this form of disease, are purgatives and blisters. As soon as the peculiar breathing is observed, I order either a smart dose of calomel and scammony, or croton oil given immediately, and a large blister applied to the scalp. If the patient be much excited, and if there be no impairment of the action of the heart, I give small doses of antimony. When the slightest appearance of comatose symptoms is manifested, I cause the patient to drink freely of strong tea and coffee. It is of great moment to apply the blister as soon as possible after the "*cerebral breathing*" is observed; in this way the scalp is in a state of irritation before coma has occurred. The blistered surface should be dressed with strong mercurial ointment. This will maintain the irritation of the scalp, and secondly, the mercury will tend to promote absorption of any effusion which may exist in the brain. When diarrhoea is present in such cases, no purgative should be given.

III. The third variety generally takes place under the following circumstances. During the course of the fever the patient does not suffer from any great amount of mental derangement; he seldom raves; he sleeps tolerably well, and has little or no subsultus. About the fourteenth day, when the febrile symptoms commence to disappear, he begins to rave, becomes sleepless, and has subsultus. Sometimes this delirious condition is but trifling, and disappears in a day or two; but, on other occasions, it becomes

much aggravated, the patient is constantly talking incoherently, his sleep is broken and disturbed, his appetite does not return, and his pulse does not fall in frequency. I have met with this variety of head affection in two classes of patients—one in which the action of the heart was much weakened, and another in which no such loss of power could be detected.

Treatment.—When the action of the heart is weakened, wine is the proper remedy, a few glasses generally prove successful. But sometimes we meet with cases of this kind of a very severe and protracted character, being always marked by want of sleep. In such instances, an opiate must be conjoined with the stimulant. Until sleep be procured the delirium will not be subdued. An opiate therefore in some shape or other must be given, to prevent the patient either sinking from exhaustion or becoming comatose.

When there is no impairment either of impulse or sounds of heart, a smart purgative, followed by nauseating doses of tartar emetic are indicated; but when intestinal irritation exists, neither purgatives nor antimonials are admissible. In some very violent instances I have found much benefit result from the application of six or eight leeches to the head. I recollect the case of a young man, who was a patient in the Royal Infirmary during the summer of 1839; he had a mild attack of typhoid; the day after the crisis he manifested great excitement, he soon became violently delirious, so much so that the use of the strait waistcoat became necessary. Six leeches were applied to side of head, his bowels were freely moved by calomel, and he had a quarter of a grain of tartar emetic every hour until sickness was induced. Under this treatment the delirium rapidly subsided, and he recovered most satisfactorily.

We therefore find cerebral symptoms appearing under two opposite conditions, one sthenic, the other asthenic. In the first, to administer stimulants would give rise to deleterious results, and in the second, to employ antiphlogistics would lead to equally destructive effects. The condition of the *heart*, however, affords a true key to the solution of the patient's condition, and enables us at once to make the necessary distinction, and to adopt the proper line of treatment. Therefore, in all such cases, we should carefully examine the heart as to its impulse and sounds, before we subject our patient to treatment. The neglect of this precaution will unquestionably give rise to very serious consequences.

IV. We have next to consider the occurrence of cerebral symptoms under totally different circumstances. In the varieties to which we have alluded, the delirium comes on at a late period of the disease; in the one to which I am now about to refer, it appears early, generally about the eighth day. The patients

who are most liable, are those of a spare habit of body, of an active, nervous, anxious temperament, and the intemperate are peculiarly prone to it. These patients often persist in attending to their avocations several days after the occurrence of febrile symptoms; indeed they only confine themselves when obliged to do so from exhaustion. About the eighth or ninth day they become restless, cease to sleep, think themselves better, become anxious about their business, so much so that they insist upon returning to it—the tradesman to his handicraft, the merchant to his counting-house, or the doctor to his patients, as the case may be. So determined are they to effect their object, that they will try every expedient to elude the vigilance of friends, and even will not hesitate to jump through the window, a melancholy occurrence which has happened on several occasions. Day and night they are in a constant state of excitement; cannot be prevailed upon to keep their bed; utterly sleepless, exhibiting considerable subsultus, indeed often, especially in the intemperate, their whole body is in a tremulous condition. This stage lasts two or three days, when the poor sufferer passes into a state of coma, from which he seldom if ever recovers.

A deep and sad interest is attached to this form of cerebral disease, from the circumstance that many medical men are lost from its effects. Unfortunately they struggle on under the disease as long as possible, before they will consent to confine themselves; then their anxiety about business, often combined with the fear of a fatal result, and reflections on the consequences which must ensue to a wife and family for whom, perhaps, little provision has been secured—these feelings completely overbalance their minds, and the condition described rapidly takes place.

Before discussing the treatment suitable to this form of delirium, I beg to direct especial attention to the important fact, that it occurs in two opposite conditions of the system—one in which great prostration exists, the skin is pale and cold, the conjunctiva bloodless, the tongue often moist, the pulse feeble and rapid, the impulse of the heart lost, and the first sound much impaired, if not inaudible. In the other form no such symptoms of vascular debility exist; on the contrary, the face is often flushed, the scalp hot, and the conjunctiva congested. Whilst, therefore, the cerebral symptoms are almost identical, the pathological conditions appear to be totally dissimilar—the one connected with great debility of the organs of circulation; and the other without any such impairment—or, in other words, at the post-mortem of one class of cases, we meet with anæmic conditions; and in the other a congested state of the vessels of the brain. The condition of the sounds and impulse of the heart

will easily enable the practitioner to diagnose the two kinds of cases; a matter of the greatest moment in reference to their respective management, to which I now refer.

Treatment.—At the onset of the attack in both classes of patients, the *strait waistcoat* is indispensable, without its assistance no amount of watching will be sufficient to prevent accidents. The struggling with nurses and friends will cause much more excitement and exhaustion than the waistcoat; and no nursing, however vigilant, will secure the patient's safety, because in a single moment a man under this form of disease may dart through the window. The strait jacket, therefore, in both the sthenic and the asthenic cases, becomes indispensable; but it is only to this extent that the treatment is uniform. In every other point it is dissimilar. The asthenic, that is, those in whom the action of the heart is impaired, as soon as symptoms of sleeplessness supervene, should have a full allowance of wine or spirits, and at night an injection of starch and laudanum; this treatment should be persisted in, not only until the delirium subsides, *but until sleep has been procured*. It must not be overlooked that sleeplessness is the first symptom—indeed it may precede the delirium for a day or two; it should, therefore, never escape the attention of the medical man: he should anxiously inquire at each visit as to the amount of sleep which the patient has enjoyed. By this plan he will often be able either to ward off altogether, or very considerably mitigate the attack, by the timely use of means calculated to procure a few hours of “balmy sleep.” With regard to the quality and quantity of the stimulant to be given, much will depend on the character of the patient. If a dissipated person, then brandy or whisky should be given, in quantities of not less than two ounces for a dose, and repeated at the end of four hours according to the effects on the symptoms. If the patient be of temperate habits, then the same quantity of wine may be ordered, and if no good result ensue, it should be repeated at the expiry of four or five hours. I prefer ordering the laudanum enema to be given at night, simply because the quietness will be more favourable to drowsiness than the bustle of the daytime. The following case is a good example of this form of the disease, and also of the effects of the plan of treatment pointed out.

Case I. (Reported by Dr. Gilland).—D. M., aged 23 years, admitted into the fever ward of the Glasgow Royal Infirmary on July 8th, 1859. Pyrexia of eight days' duration—a slight typhus eruption exists over trunk and extremities. He is delirious, talks incoherently about his business, refuses to remain in bed, and he is anxious to get home; pulse weak, 120; impulse of heart feeble, and first sound faint and short; face pale; tongue brown and dry;

skin moderately warm ; subsultus very considerable. The strait jacket was put on, his head shaved, and a blister to nape of neck. On the 9th, I found that he had passed a sleepless night, his pulse was more feeble, and he seemed very much prostrated. He was ordered to have an ounce and a half of whisky every three hours, and to have an injection of forty drops of laudanum at bedtime. On the 10th, I found that he had slept little—the raving still continued, and the subsultus increased; his whisky was increased to two ounces every four hours, and he was ordered in addition two grains of camphor, and two grains of extract of hyoscyamus every six hours, and the laudanum injection to be given at bedtime. On the 11th, I found that he had slept soundly several hours during the night; he was much improved, less subsultus, less raving, and the action of the heart greatly better. The whisky was ordered to be reduced to one ounce every four hours, the pills to be continued, but the injection to be omitted. On the 12th he was much improved, had slept well, subsultus reduced, and much less delirium. The whisky was discontinued and two ounces of port wine substituted every six hours. From this time he gradually improved, and on the 14th a favourable crisis occurred. His wine and other remedies were discontinued, and his convalescence progressed with rapidity.

In this case the beneficial effect of the stimulating plan of treatment, was not more strongly marked than I have usually witnessed in such patients. Whenever delirium is associated with impaired action of the heart, no matter how violent the patient may be, I consider that the free use of stimulants is the only method by which a favourable result can be secured.

A very different plan of management is demanded when the delirium is combined with a normal condition of the heart; in these instances the influence of tartar emetic is of paramount importance. Stimulants would in such patients be productive of the most serious results. Dr. Graves, though not the first to use antimony in this form of delirium, yet brought its beneficial effects so prominently before the profession that we are justified in denominating the plan of treatment by his name.

When I find a patient becoming sleepless or delirious about the eighth or ninth day, and when I ascertain that the state of the action of the heart is unimpaired, I order the *strait waistcoat* to be applied, and put him under the use of antimony. Dr. Graves added a few drops of laudanum to each dose. I have not done so for many years, as I have found the antimony to be as useful without as with the laudanum. It is sometimes difficult to get patients to take anything in the shape of medicine; I therefore prefer giving the antimony in the ordinary watery solution (of two grains to the ounce); and, as this is devoid of

taste and colour, it can be added to the patient's drink of water, which is seldom declined, but on the contrary taken with avidity.

The Dose.—With regard to the dose of the antimonial solution, I generally order a large teaspoonful to be given every hour until sickness is produced; after this I either reduce the dose, or order it at longer intervals.

Cautions to be observed in the use of Antimony.—1st. It may depress the action of the heart too much. A careful examination of the state of the impulse and sounds of this organ will obviate any risk on this point. If too much should be given, an ounce or two of brandy will remedy the evil.

2nd. The antimony may produce irritation of the intestinal mucous membrane. In all cases in which diarrhoea or other abdominal symptoms exist, the drug must be used with great caution; and in such instances the addition of a few drops of laudanum to each dose will be useful. As soon as the tongue becomes dry and red, or the bowels loose, the solution should be either withheld or given in small quantities. In such exigencies I have often used the *Pulv. Jacobi ver.* instead of the solution. Indeed, when this form of delirium is associated with abdominal symptoms, I prefer ordering the James' powder in doses of two or three grains every two hours, and often with decided benefit.

3d. It is necessary in all cases in which antimonials are employed, that the medical man should visit his patient at least twice daily, if not oftener. Life is in the greatest peril, and neither nurse nor friend should be allowed to have any discretionary power in the administration of this remedy. The medical attendant must undertake the sole responsibility himself, and consequently must see his patient frequently, so as to watch carefully its effects.

With these precautions, I contend that, in the class of cases under consideration, the antimonial treatment is not only devoid of all danger, but is attended with the happiest results—effects which cannot be secured by any other plan of treatment. I beg to narrate one or two cases illustrative of its power.

Case II. (Reported by Dr. Gilland).—A. H., aged 34 years, by trade a blacksmith, admitted to the fever hospital on the 15th July, 1859. On admission it was found that he had laboured under febrile symptoms seven days. He was not long in the ward till he became delirious, wanted to get away, would not remain in bed, and attempted to go out of the window. His skin was covered with a profuse perspiration, and over his whole body a typhus eruption existed; tongue furred and tremulous; face slightly flushed; state of bowels could not be ascertained; pulse 120; action and impulse of heart vigorous.

He was confined to bed by the *strait jacket*, and had a *teaspoonful of the antimonial solution administered to him every hour*. The first two doses were vomited, but afterwards they were retained. After the sixth, the impulse of the heart became feeble, and the patient much quieter. The antimony was suspended. During the night he did not sleep, and the next morning (the 16th) the delirium returned with fury, while the action of heart was restored. The treatment was resumed, and continued till next morning (the 17th); he had no sleep, but the raving was greatly abated. The solution was ordered to be given every four hours, and if no sleep ensued, a starch and laudanum injection to be given at bedtime. In the evening weakness of the heart's action supervened; consequently, the antimony was suspended, and the laudanum injection administered. During the night he was quiet, but talked incoherently, and seemed to slumber only a few minutes. On the 18th the delirium returned, and with it improved action of heart; the use of antimony was resumed, and after three doses he fell into a sleep, which lasted several hours. The antimony was discontinued; and on the morning of the 19th he was very well indeed; the delirium gone; the strait jacket was removed, and he again fell into a sound sleep, from which he awoke quite collected. From this time he progressed most favourably, until perfect health was established.

In this case there are some points worthy of attention—1st, the depressing influence of the antimony on the heart, necessitating the discontinuance of the remedy several times; 2nd, the importance therefore of the medical man carefully watching his patient whilst under the influence of the remedy; 3d, the effect of the remedy in allaying delirium, and the return of this state shortly after suspension of the solution; 4th, the total uselessness of the starch and laudanum injection in procuring sleep—(this I have found to be the general result in such cases, a result very different from that which takes place in those instances in which an asthenic condition exists); 5th, the non-subsidence of the delirium until the patient slept.

I again repeat that though the delirium in such cases should even disappear, yet unless the patient sleeps the strait jacket should not be removed; because in the great majority of instances the delirium will return, often indeed with increased violence. But this relapse very seldom occurs if the patient obtains several hours' sound repose.

Comatose Symptoms.—I have already stated that in these patients, if the delirium and sleeplessness be not subdued, that coma will supervene. On the approach of such symptoms, a blister should be applied to scalp; but if this should be applied earlier in these cases, the irritation which it produces will tend

to aggravate the delirium and prevent sleep; its application should therefore be reserved until symptoms of stupor make their appearance. I prefer blistering the upper part of scalp, to the nape of neck or posterior part of head, so as to avoid extra annoyance to the patient, who generally lies on his back. I am also in the habit of applying a good large blister.

There is another precaution to which I beg to direct attention in these cases; namely, when the patient falls asleep he should not be allowed to continue in that state longer than three or four hours at a time; there is considerable risk of the sleep passing into coma. I therefore always cause the patient to be roused up at the end of a few hours, and to have a cupful of tea or coffee. If in this semi-comatose condition the action of the heart should become impaired, I order an ounce or two of brandy to be given in the tea every three or four hours, until the condition of the heart improves. I prefer brandy, because it appears to me to act more quickly than wine, and its effects on the brain are more transitory than those of whisky.

V. I have next to direct attention to another class of cases which occurs among the young and robust, especially those who follow agricultural pursuits or live in the country. From the very outset of the disease these patients complain of the most intense headache; their suffering is so great that they can neither sleep nor lie in the recumbent position. They will often be found sitting on the bed, their head supported either on their hand or knees. When they fall into a slumber, they will sometimes start up in agony, under the impression that some person has struck them severely on the head. I recollect one patient who, under this belief, leaped out of bed, seized a stick, and ran through the house in pursuit of the supposed inflicter of the blow. On the fifth or sixth day they become delirious; they are perfectly furious; great force is required in order to restrain them, and it is often a very difficult matter to get the strait jacket applied. I have seen their wrists much lacerated in their struggle to get free. One patient very nearly strangled himself in the attempt. The face is flushed, the conjunctiva red, eyes ferrety, tongue dry and red. In a few days they become comatose, this condition being often preceded by cerebral breathing; their pupils become dilated; urine and alvine evacuations passed in bed. Their lips, teeth, and gums become covered with sordes.

The post-mortem examination reveals severe cerebral congestion, and often effusion into the ventricles.

Diagnosis.—This class of cases is easily distinguished from the others by the intensity of the headache, the early occurrence of the fierce delirium, and the congested condition of face and eyes. Such cases have not unfrequently been denominated "brain fever,"

and not inaptly so, as from the very outset of the attack the fever poison seems to exert its full power in the brain.

Prognosis is very unfavourable; a fatal result often rapidly ensues. Unless active measures be adopted early, the patient will seldom recover: often, indeed, the most prompt and judicious treatment proves useless.

Treatment.—This will depend on the period of the disease. For practical purposes the attack may be divided into three stages—1. That prior to the occurrence of delirium; 2. The delirious stage; 3. That of coma.

1. When a young robust patient is suspected to be seized with fever, if he complain of intense headache, preventing sleep, and becoming so aggravated on lying down as to cause him to sit up, and these conditions accompanied with hot skin, flushed face, and reddened conjunctiva—the scalp should be immediately shaved, and from six to twelve leeches applied to temples or nape of neck. This should be followed by cold applications to scalp. The bowels should be freely moved by an active purgative; perhaps the sulphate of magnesia is the best one to select, in consequence of its *modus operandi*. I very often order a mixture composed of three ounces of sulphate of magnesia and sixteen ounces of infusion of roses; of this a wine glassful is given every three hours until the bowels are freely moved. A smart dose of calomel and scammony also acts well, and in some very acute cases I have given with speedy results a drop of croton oil.

The employment of such remedies by one who has advocated the views regarding the state of the intestinal mucous membrane in fever that I have done in this series of papers, requires some explanation, in order to obviate the charge of inconsistency which might be urged by those who take a superficial view of the case. I beg to remark, in the first place, that the intestinal mucous membrane does not become affected until *the third or fourth day of the attack*; hence there is no risk of increasing that which does not exist.

2nd. It must be recollected that I am speaking of the treatment of a special class of cases, at a period prior to that at which the intestinal congestion is usually developed.

3rd. In this class of cases the fever poison seems to attack the brain with great intensity, forsaking its customary site of development, precisely in the same way that the poison of small-pox and scarlatina not unfrequently attack the brain, the skin remaining almost free from the usual manifestation. The purgative plan therefore, in the earliest stages of the form of fever under consideration, may *prove of paramount importance*, by determining to the mucous membrane of the alimentary canal, and in this way relieving the brain from its undue and unusual share of the

malignant influence; on the very same principle that, by acting on the skin, we endeavour to relieve the brain in the exanthemata when severe congestion of that organ occurs.

These considerations being quite sufficient to meet any charge of inconsistency which may be urged against my views, I beg to resume the subject of treatment: Besides bleeding and purgatives, the patient should be kept perfectly quiet, free from every cause of mental excitement. The apartment should be kept cool, darkened, and protected as much as possible from noise. The head should be kept elevated, and constantly cool by cold sponging or the application of broken ice in a bladder, or some similar appliances. Such is the plan of treatment which I have found most beneficial in these cases, and the effect has not unfrequently been so salutary as either to prevent, or very considerably mitigate, the stage of delirium, to which I now beg to direct attention.

II. *Treatment of the Stage of Delirium*.—If bleeding has not been previously adopted, a few leeches to the temple should be applied, and a purgative administered. The chief remedy, however, is tartar emetic. This must be given freely in the manner already described. In this class of cases its influence is most decided; generally six or seven doses quiet the excitement, but do not always procure sleep. When, however, the delirium has been subdued, a laudanum injection will secure sleep. Care must be taken not to give the opiate during the existence of delirium, which it will aggravate. In this class of cases it is advisable, when sleep does not follow the subsidence of the delirium, to continue the use of the tartar emetic solution in small doses, because there is a great tendency to relapse, unless the patient has slept. The same precautions must be taken regarding the effect of the antimony on the heart and intestinal mucous membrane, as has been pointed out previously. The state of the bladder should be attended to in such patients. They soon become insensible to the calls of nature. The urine usually accumulates; the bladder becomes distended, and then the urine dribbles away in drops. Whenever any fullness is felt above the pubes, the catheter should be employed.

III. *Treatment of the Comatose Stage*.—When symptoms of coma begin to be manifested, a large blister should be applied to scalp. Stimulants are often valuable when these patients get into a semi-comatose state, but the action of the heart must be the guide in their administration. When the cardiac impulse becomes feeble, which it occasionally does, there is some risk of effusion into the brain. The vascular excitement which has existed for some days will pass into a state of stasis when the *vis a tergo* becomes feeble, and hence exudation of the watery parts of the blood may occur.

Therefore, when any failure of the impulse of the heart is detected, a stimulant should be given. Tea or coffee may be tried at first; these will often succeed, but, if they fail, then an ounce or two of brandy should be ordered; it may be given in either the tea or coffee. By this plan I have often prevented the patient from falling into profound coma.

During the year 1859 five cases of this acute form of delirium were admitted into the Royal Infirmary. The patients were all farm-servants from the neighbourhood of the city. One died comatose on the second day after admission; indeed he was perfectly insensible when he was admitted. Another was admitted also in profound coma; he likewise died, but not till four days after admission. The other three were admitted at the commencement of the delirious stage; they recovered. The following history of these instances will illustrate their character and the effects of the treatment which I have recommended.

Case III. (Reported by Dr. Gilland.)—C. M'L., aged 19, a farm-servant, admitted 14th November. Said to have been seized with febrile symptoms six days previously; he is in a state of the most violent delirium; cannot be kept in bed, attempts to escape by the window, strikes violently at those who attempt to restrain him, has not slept for forty-eight hours, and has complained of intense headache. His face is much flushed; eyes suffused; impulse of heart good, and sounds normal; pulse 120, of good strength. The strait jacket was put on with great difficulty, and he was ordered a teaspoonful of the antimonial solution every hour. His head to be shaved, and to be kept constantly cold. After taking nine doses he became quieter. On the 16th he was very stupid; could not be got to answer questions intelligently, but lay with his eyes half shut; pupils dilated. The breathing was "cerebral," and expiration attended with a "puff." The antimony was suspended, and a large blister applied to the greater part of scalp; he was ordered to have three grains of James' powder and one of calomel every four hours. On the 17th the bladder was detected to be in a state of distension; the catheter was used, and a large quantity of urine taken away. On the 18th he was much in the same condition; the pupil of right eye was contracted, whilst that of the left was dilated. The blister, which had risen well, was ordered to be dressed with strong mercurial ointment. From this time he commenced gradually to improve. On the 20th his gums were slightly tender, and by the 25th he was quite sensible, and able to evacuate his bladder without the assistance of the catheter. On the 8th December he was dismissed well.

In the second case the patient was 22 years of age. He was admitted on the seventh day of his illness in a state of fierce deli-

rium. Under the influence of the antimony this abated in the course of the second day, and he afterwards progressed favourably. The third patient was admitted on the fourth day of the fever, labouring under severe headache, preventing sleep, indeed he was obliged to sit up from the aggravation produced when he placed his head on the pillow. Six leeches were applied to temples, and his bowels were freely moved by sulphate of magnesia. On the second day after admission he became delirious, but not quite so furious as the others; he had six doses of the antimonial solution, when he became quiet, but sleepless. An injection of laudanum was given; this procured a little sleep, but next day the delirium returned, and was followed by stupor and weakness of heart's action. He was ordered an ounce of brandy in a cupful of tea every three hours. After the second quantity the action of heart improved; the brandy was withheld, and he afterwards progressed favourably. In the fatal cases the treatment consisted of blisters to stomach but both instances were hopeless when admitted. I may mention that the typhus eruption in four of these cases was very extensively diffused over the whole body. In one of the fatal cases no spots were detected.

Cases of this variety of delirium will be seldom if ever seen among the inhabitants of large towns, or among the old, the poor, or ill-fed part of the community. It is among the young robust country people that it makes its appearance, and even among this class it is by no means frequent—seldom during epidemics; chiefly when the disease is sporadic. Hence such cases are liable to be confounded with non-specific cephalitis. A few words therefore on the diagnosis may not be inappropriate:—1. The appearance of the eruption, when it exists, makes the distinction easy. It should not be overlooked that, generally, the symptoms of delirium set in about the time at which the eruption commences to appear: consequently, the skin should be examined daily. 2. The fact of recent exposure to contagion will also assist the diagnosis. 3. In fever, subsultus is more severe than in ordinary inflammation of the brain. 4. There is seldom the peculiar vomiting in fever which we find so often in inflammation of the brain. 5. In fever there is an amount of tremulousness about the patient's tongue and extremities, and a constant restlessness which we seldom witness in cephalitis. In fact, this acute delirium of fever much more resembles that of delirium tremens than that of inflammation of the brain. However, in many cases the diagnosis is beset with great difficulty, and requires much discernment, accurate observation, and a comprehensive consideration of every circumstance connected with the history of the case. When doubts still exist, a cautious practice should be adopted until the character of the attack becomes more clearly manifested.

II.—LESIONS OF THE RESPIRATORY ORGANS.

These affections are exceedingly frequent, and merit the greatest attention. In almost every case of typhus, we have congestion of the mucous membrane of the bronchial tubes developed to some extent. This is obvious from the sonorous and sibilant rales which are heard over the chest during the earlier stages, and the mucous ronchus which is often heard towards the termination. Such simple congestions usually disappear at the termination of the febrile symptoms, and require no special treatment. The lesions to which I am about to refer are of a much more severe character, and require the most prompt and judicious management. We have—

1. Extensive and severe congestion of the lungs.
2. Pneumonia and Pleuro-pneumonia.
3. Deposit of Tubercles.

1. *Extensive Pulmonary Congestion.*—This is a most fatal complication. It constitutes that form of typhus denominated “congestive” by some writers. The breathing becomes rapid and oppressed; the face becomes tumid, congested, and, in a very short time, livid. Mucous ronchii become developed, and are heard at a considerable distance from the patient’s bed. The skin becomes cold, and the hands often livid. The pulse weak and unsteady. The patient soon sinks into hopeless coma.

Treatment.—This congestive affection of the lungs may arise in conjunction either with weakened action of the heart, or quite independent of this complication. It may also supervene after the development of cerebral disturbance. The treatment must be modified in accordance with these circumstances. When the impulse and sounds of heart are impaired, then stimulants must be administered freely. No other remedy will save the patient. When no such impairment can be detected, then dry cupping the chest and a mild purgative (if no abdominal lesion be present). Next sinapisms to the chest, followed by blisters, constitute the treatment. I have not witnessed many recoveries when this congestive condition of lungs was associated with cerebral excitement; those who did survive seemed to have been benefited by large blisters to occiput and nape of neck. On a post-mortem examination of several cases I have found extensive congestion of the vessels of the lungs, and effusion of bloody serum into the air cells and bronchial tubes, but no appearance of hepatization. This form of pulmonary lesion usually supervenes during the second week, sometimes about the seventh day—more frequently, however, about the eleventh—and is seldom associated with severe abdominal lesion; indeed, a fatal issue generally takes place before the stage of intestinal ulceration has become developed.

I repeat that it is by far the most formidable of the pulmonary lesions which I have witnessed in fever, and when it has existed for any length of time becomes perfectly unmanageable.

2. *Pneumonia and Pleuro-Pneumonia* are often developed during the progress of typhus, especially if the patient has been exposed to cold at the commencement of the attack. These complications are more frequent during the winter and spring months than at other times. But in some epidemics there seems to exist a very peculiar liability to these diseases. I recollect that, during the winter and spring months of 1852-53, this circumstance was strikingly witnessed at the Fever Hospital. For example, in the month of February, out of fifty-eight patients admitted into the wards under my care, twenty-four were seized either with pneumonia or pleuro-pneumonia; and in the month of March, out of fifty admissions, we had twenty-four attacks of the same pulmonary lesions.

These complications were not only frequent, but they were most severe, extensive, and exceedingly rapid in their development. For example, a patient in whose chest no abnormal symptoms could be detected at the hour of visit, before next day would have the whole of one side perfectly dull on percussion, with all the other symptoms, either of pneumonia or pleuro-pneumonia. In many instances, in the course of a few hours, the greater portion of both lungs became involved. In such rapid and extensive cases death invariably resulted; when the disease was less severe, stimulants were the only remedies which proved useful. It is not, however, such rapidly severe cases of pulmonary lesions which we meet with under ordinary circumstances. The following instances afford an example of the more common forms:—

Case IV.—(Reported by Dr. Gilland).—Thomas W., aged 23, admitted to the Fever Hospital, May 25, 1859. Seven days previously he was seized with rigors, followed by pain of head and febrile symptoms; a few days afterwards he was attacked with pain in chest, cough, and difficult breathing, and expectorated some bloody-looking matter. On admission, he had a weak and anxious appearance; he had also a very dusky hue of countenance; his respirations were rapid, short, and gasping; skin cool; tongue coated and dry. On percussing chest there was great dullness found over the posterior regions of right side of chest, and also in the lateral and anterior regions, but not to the same extent. Dry crepitation was heard at several parts anteriorly and laterally, and bronchial breathing and bronchophony detected over dull portions. Heart's sounds and impulse exceedingly feeble; pulse 144, weak and compressible; typhus eruption extensive over chest and abdomen; bowels easy; urine scanty and high-coloured.

Treatment and Progress.—The right side of chest was dry-cupped on the 26th for an hour; a sinapism was afterwards applied, and he was allowed one ounce of whisky every two hours. Next day the action of the heart was much improved; a blister was applied to right side of chest; an ounce of wine every two hours was substituted for the whisky. On the 28th he was still further improved; the blister had risen well. The blistered surface was ordered to be dressed with mercurial ointment. On the 31st the febrile symptoms disappeared, and the state of the chest was so much improved, that all treatment was suspended. He gradually recovered, and was dismissed on the 14th June perfectly free from every trace of disease of lungs.

This case affords a very good example of one form of pulmonary lesion met with in typhus; namely, extensive hepatization, associated with *impairment of the power of the heart*. In such cases *stimulants* form the most suitable, indeed the only advisable treatment. Dry cupping and blistering are also useful, but merely as adjuvants. In some cases, however, the quantity of stimulants must be much greater than that used by this patient. For example the following instances:—

Case V.—(Reported by Dr. Gilland).—P. M. C., aged 17, admitted 20th September. Pyrexial symptoms of nine days' duration. On admission, patient restless; has great difficulty of breathing, and constant cough. On examination of chest, dullness over greater portion of left lung detected, with bronchial breathing and bronchophony; heart's action weak; impulse exceedingly slight, and first sound feeble; pulse 120; tongue coated and dry.

Treatment and Progress.—He was ordered a sinapism to chest, and to have one ounce of whisky every six hours. Next day little change; but on the 22nd a friction sound was detected over the left mammary region; action of heart more feeble; whisky increased to one ounce every two hours. On the 24th he was somewhat improved. On the 25th so much better that the whisky was reduced to one ounce every six hours, and a blister applied over left side of chest; on the 27th he was so much better that the whisky was suspended, and the blistered surface was ordered to be dressed with mercurial ointment. He was dismissed quite well on the 21st October. There remained, however, some dullness over lower part of left infra-mammary region, and some retraction of the intercostal spaces was observed.

Case VI.—R. B., aged 47, admitted 28th October, 1857. Pyrexia of eight days' duration; skin hot and pungent, and covered with a typhus eruption; pulse 110, full, but jerking; tongue much furred; respiration hurried and difficult; cough severe; expectoration rusty. On examination of chest, dullness

over base of each lung, both anteriorly and posteriorly; sonorous and sibilant rales abundant over other parts of chest; bronchial breathing and bronchophony over dull portions; impulse of heart faint, and first sound nearly inaudible.

Treatment and Progress.—On the 29th he was ordered to have an ounce of port wine every four hours. On the 31st he was much worse; his breathing was very much oppressed; countenance dusky; pulse 120, feeble; dullness extended over nearly the whole of the right lung; first sound and impulse of heart almost undistinguishable over left apex. He was ordered to have, in addition to the wine, an ounce of whisky every four hours. Next day he was found partially comatose. The whisky was increased to two ounces every four hours. On the following morning the condition of the heart's sounds and impulse was improved. On the 3d November he was greatly better; the wine was discontinued; and on the 5th he was so much improved that the whisky was reduced to an ounce every six hours. On the 7th it was discontinued; and on the 19th he was dismissed quite well. No traces of pulmonary disease could be detected.

It will be observed that, in these patients, the action of the heart became very weak; it was in consequence of this failure that the stimulants were so freely employed. This is, indeed, the only plan of treatment which should be adopted in cases of this character. Whenever the impulse and sounds of heart become feeble, then it matters not how extensive the pulmonary lesion may be, stimulants afford the only means by which the patient's life can be saved.

But we meet with another class of pulmonary cases in which no cardiac impairment exists. In such instances stimulants would prove injurious; on the other hand, mild antiphlogistics are demanded. The following case is an example of this kind:—

Case VII.—(Reported by Dr. Gilland).—F. M., aged 18 years, admitted 30th May, 1859. Fever of eight days' duration; face flushed; skin hot and dry; thirst; tongue furred; respiration quick and laborious; cough troublesome; expectoration rusty and tenacious; on chest and abdomen there exist a few rose-coloured papulæ, and pressure elicits pain over right iliac region; some gurgling is also detected. On examination of chest, extensive dullness is found to exist on percussion over the whole of left lung. At the upper region, near the clavicle, the dry crepitant rale is heard, and bronchophony and bronchial breathing detected over dull part. States that affection of chest commenced a few days after date of illness. Heart's sounds normal; pulse 104, full; bowels loose, and dejections frequent.

Treatment and Progress.—A large blister was applied to left side of chest, and he was ordered to take two grains of the ace-

tate of lead, and half a grain of powdered opium every six hours. The blistered surface was ordered to be dressed with mercurial ointment. This treatment was continued until 6th June, when the patient was very much improved in every respect. Dullness, however, still existed, but to a much less extent. A blister was applied to anterior part of chest, and the acetate of lead omitted. He recovered perfectly, and no trace of pulmonary disease could be detected when he was dismissed.

In this case we had also abdominal lesion to manage—a combination by no means uncommon, and which led me, many years ago, to observe the rapid disappearance of the hepatization of the lungs under the use of the acetate of lead, given with a view to check the intestinal lesion. This circumstance has induced me to employ the remedy in cases of pneumonia, either occurring in the course of typhus (whether combined or not with diarrhoea), or in other low conditions of the system. The result has been so beneficial in my experience, that I can recommend the drug in similar cases with the greatest confidence to my medical brethren. In confirmation of its efficacy, I may mention that, very recently, some continental writers have spoken strongly in favour of the acetate of lead in the treatment of pneumonia, whether complicated or uncomplicated. For instance, Professor Strohl of Strasburg states that “after trying the various remedies in pneumonia, he has come to the conclusion that the acetate of lead, given in full doses, is the best. When, however, *there is plethora*, he bleeds once, but rarely twice; and when this is absent, he only cups or applies leeches. Soon after the acetate has been commenced, the pulse diminishes in frequency, but the local symptoms for a while continue to extend; but this is only for a time, after which they ameliorate, and the acetate should be suspended. Convalescence takes place at the end of from five to twelve days, but it is a very complete one, the strength rapidly returning. This treatment is suitable for all the forms of pneumonia, and for all ages at which these appear.”—*Gaz. des Hôp.* 1861, No. 1; and *Med. Times and Gaz.* January 19, 1861.

Whilst I can testify to the efficacy of the remedy in asthenic cases of pneumonia, either occurring during fever or under other debilitating circumstances, yet I do not feel so certain as to the propriety of trusting to it in acute uncomplicated cases. It will be observed that Professor Strohl employs bleeding in such cases prior to the use of the acetate of lead; in fact he *reduces* the power of the organs of circulation, and to some extent places the patient in an asthenic condition. But apart from such cases, I feel assured that, at least in the pneumonia associated with typhus, acetate of lead will be found to have a most excellent effect.

In conclusion, I beg again to remark that the pneumonia of typhus will be found to take place under two conditions of the organs of circulation — in one of which there is failure of the heart's action, and another in which no impairment can be detected. The treatment, consequently, must be based on the state of the heart. If weak, then stimulants must be freely and promptly employed. In the other class of cases, mild antiphlogistics, combined with the acetate of lead, will be found to form the most suitable plan of management. The condition of the impulse and first sound of heart forms, therefore, the key to the treatment in the pulmonary complications of typhus, just in the same way as it does in the cerebral and intestinal lesions.

There is another form of pulmonary disturbance, to which I now briefly advert. I have only occasionally met with it. It arises during the course of the cerebral affections previously described. The breathing becomes quick as coma approaches, and symptoms of pulmonary congestion are rapidly manifested, while extensive effusion takes place into the air vesicles and bronchial tubes. The treatment suitable for the disorder of the brain is equally applicable to this pulmonary affection, namely, blister to the head and nape of neck. This combination of cerebral and pulmonary lesions is a very unfortunate one; such cases generally prove fatal.

3. *Deposit of Tubercle*.—I do not intend to enlarge upon this point, but merely to remark that I have found an attack of fever frequently followed by the rapid development of tubercle in the lungs of those who had a hereditary or acquired tendency to this affection. Such patients, therefore, should be carefully watched during their convalescence, should receive a better diet, and be sent for some weeks or months to a moderate climate. Patients labouring under incipient tubercle, when attacked by typhus, often succumb shortly after the subsidence of the fever, in consequence of the rapid softening of the tubercle; this class of people consequently demand especial attention both during, and subsequent to the attack of fever.

In concluding this series of papers on the pathology and treatment of fever, I beg to observe that, though I have at some length endeavoured to prove that the typhus and typhoid forms of the disease are identical as to cause—that is, both arise from the same morbid poison—and that the theory of their non-identity is founded on erroneous conclusions, deduced from imperfect observations and mere conjectures; yet I beg to state that this has not been my principal object. My chief design has been to direct attention to the *clinical history of fever*, and to advocate a treatment founded in accordance with bedside *facts*, which are capable of

demonstration. Neither the doctrine which prevails as to the cause of fever, nor the theory which at present seems to be generally adopted in explanation of that doctrine, is of the slightest value in the treatment of the disease. Though we believe that fever arises from a morbid poison absorbed into the system, yet this knowledge does not enable us to cut short the malady, and has indeed very little influence, if any, on the treatment.

With regard to the theory of Liebig, that this morbid poison acts as a ferment, I beg to state that it is an hypothesis which is totally unsupported by a single apposite fact, but, on the contrary, is opposed to well-established principles. For example, if fermentation, or any process analogous to it, should be set up in the blood, such a process could never come to an end during life, in consequence of the ever-changing character of the blood; the position of this fluid being perfectly dissimilar to that of the fixed quantity of saccharine fluid in the brewers vat. Even here let a stream of this fluid be constantly escaping from the vessel, and a new portion be as constantly added to it—why, fermentation would never cease so long as these changes were in operation. This is precisely the case regarding the blood in the bloodvessels, and it appears to me perfectly inexplicable how that, with this *fact* so prominently patent to all, the theory of the distinguished chemist should have been entertained for a single moment by physicians; and it is still more astonishing that the theory should have had the effect of changing the nomenclature of the whole class of diseases arising from morbid poisons. The term *Zymotic* is a standing reproach to the medical profession, because it shows how easily we have been captivated by a mere theory, and thereby so blinded as to overlook the well-established laws and facts connected with the process of fermentation. But, apart from considerations regarding the truth of this hypothesis, even supposing it to be correct, let me ask what advantage does it confer in the treatment of the disease? Does it suggest the shadow of an idea as to the management of a single case, or the removal of a single pathological condition which exists? Assuredly not. On the contrary, the adoption of this zymotic theory positively may do much harm; it tends to paralyze all investigation into the pathological states which actually occur. The notion that a process of fermentation must be *completed* leads the practitioner to let the disease run its course, without an effort to ward off or remove those lesions, both of function and texture, from which death results. We have seen that death in fever very seldom, if ever, arises from mere functional disturbance; but invariably, or very nearly so, from actual structural changes—viz., congestion or exudation into the brain, softening and

degeneration of the muscular tissue of the heart, ulceration, and many other associated lesions of the small intestines, or from serious disease of the pulmonary organs. Let me inquire how fermentation produces such morbid results in solid tissues? As practical physicians we must discard all theory in the treatment of fever as a mere "will o' the wisp" which will lead us astray. We must base our treatment on the knowledge which we obtain in our post-mortem examinations, combined with careful clinical observations made during life. The facts so obtained must be our sole guide, and we must never neglect their valuable instruction. They teach us the following important lessons:—1st. That typhus produces an effect upon the whole frame; that the muscular system, the brain and nervous system, the circulatory system, the digestive or nutritive system, and the respiratory system, are all more or less deranged. 2d. Besides this, observation teaches us that persons attacked by typhus are not uniformly influenced. 3d. We see that all the different functions or systems are not uniformly attacked with equal severity, but that, in some cases, and during some epidemics, one series of lesions are more frequent or more severe than in others; that whilst, as a general law, disease of the mucous membrane of the intestines is the usual pathological manifestation of the disease, yet diseases of other organs and other tissues frequently supervene, and prove destructive to life.

Let me next point out very shortly the practical value of these clinical facts in the treatment of typhus:—1st, The fact that typhus produces an effect on the whole system, leads us to direct attention to the condition of every organ in the body. The muscular system becomes impaired; we therefore must take care to prevent exertion; we should confine the patient to bed; cause him at an early period to use the bed-pan and the urinal. Though he may be perfectly able to get up to perform these necessary evacuations, yet as the time may come when he will be unfit to do so, it is a good plan to accustom him to the use of these utensils whilst he has sense to guide him; and besides this, when care is taken to preserve the patient's strength in the early stages of the disease, the subsequent prostration will be modified.

2nd, The knowledge that the digestive system is impaired points out the necessity of great attention being devoted to the character of the food and drink, so as to give the patient aliment best calculated to undergo easy digestion. Again, the almost invariable implication of the mucous membrane of the small intestines in the disease, demands that every care should be taken to avoid the exhibition of any article of food or medicine which is likely to produce irritation of the alimentary canal.

The knowledge of the existence of this intestinal lesion also guides us to the early employment of means, calculated either to modify the extent of its development or remove its effect.

3rd, The fact that the brain is more or less affected, points out to us the necessity of taking steps to prevent all causes of excitement. The patient should not be annoyed with business matters; there should be no conversation allowed in the sick-room calculated to excite his mind; the head should be shaved and kept cool by cold applications; the bed-chamber should be kept quiet, and somewhat darkened. Those symptoms to which I have referred as indications of severe cerebral disturbance should be carefully attended to—such as sleeplessness, subsultus, cerebral breathing; because it is only at the earliest stages of these complications that we can hope to secure success from treatment.

4th, The knowledge that the respiratory system is also affected in fever, leads us to adopt measures to maintain a constant supply of pure air in the patient's apartment, avoiding all currents, and cautions us to make a careful examination of the chest at every visit.

5th, The fact that the heart becomes diseased in fever instructs us to make it a principal point in our attendance to ascertain frequently the state of its impulse and sounds, so as to be able to avail ourselves of this only safe key to the administration of stimulants.

6th, The evacuations must also receive a careful attention. The peculiar loose stool is most significant; and the important duty of attending to the state of the bladder must never be overlooked. These are points which should never be left to the nurse. The practitioner must always institute a personal examination.

But the clinical fact that fever affects the whole system is not of greater practical value than the other fact, that these different systems are neither equally nor uniformly disordered. The knowledge of this important circumstance leads us to examine every case with the greatest care, so as to be able to detect at the very earliest moment any tendency to the undue disturbance of any particular function or organ.

These facts instruct us to watch carefully, not only the peculiar manifestations in each individual attacked with fever, but also the tendencies which are exhibited in different epidemics to affect some organs more than others.

Whilst we know that, as a general law, the fever poison impairs the whole system, and that it produces its local manifestation on the mucous membrane of the small intestines, we also know that it attacks other tissues and organs secondarily, and that these secondary lesions are by no means either constant or of uniform intensity, but that at the same time they are frequently much

more severe than the primary intestinal ones, and often prove more fatal.

It must follow from these considerations, that fever cannot be correctly treated by any *routine practice*; on the contrary, it is perfectly clear that whilst every attention must be given to the usual anatomical lesion in every case, yet each individual patient must be treated according to the character of his constitution, and the nature and extent of the various secondary complications to which I have directed attention in this series of contributions—papers which I regret to say abound with many imperfections. The only apology which I can plead is, that they have been written at considerable intervals, amidst the usual interruptions of professional life.

But whatever faults may be detected, I beg to remark, that I have endeavoured to place before the readers of this *Journal* the result of nearly twenty-five years' experience of a disease in which I have always taken a deep interest, a disease which I have had ample opportunities of studying under all circumstances, viz., in the rural district, in the manufacturing and mining village, in the overcrowded and densely populated city, in the private dwelling, and in the public hospital; in its sporadic forms, and in its epidemic outbreaks.

V. *Description of a French Instrument for effecting reduction or reposition of the Umbilical Funis when prolapsed, with cases illustrative of its successful application.* By J. G. WILSON, M.D., F.R.C.S.E., Fellow of the Faculty of Physicians and Surgeons, Physician to the Glasgow Lying-in Hospital, Fellow of the Obstetrical Society of London, &c.

THERE are few complications occurring in the course of parturition, which to the accoucheur occasion more anxiety, and involve more trouble as regards management, than prolapse of the umbilical cord before or along with the presenting part of the child. Such a complication is one fraught with imminent peril to the infant, without endangering in the least degree the safety of the mother, or interfering in any way with the progress and duration of the labour. Prolapse of the cord is an accident fortunately of comparatively rare occurrence. According to the statistics of Dr. Churchill, out of 123,224 cases of all presentations collected by him, there were 579 funis cases, or about 1 in 221½. As regards the infantile mortality attendant on cord presentations, consider-

ably more than 1 in 2, or upwards of one half of the children perish—a higher mortality than we find in any other variety or order of labour. Hence it follows, and it is a matter of general experience, that the modes or methods of treatment in such cases, numerous and varied as they are, which have hitherto been recommended, are in a great measure futile and inefficient, as far as the salvation of infantile life is concerned. The same remark applies to the various mechanical means and contrivances, however ingenious they may seem, which have been from time to time devised for rectifying this displacement of the cord. Amongst the more ingenious and feasible mechanical contrivances designed for replacing the funis, may be mentioned the instruments of Chailly, Ramsbotham, Murphy, Michaelis, and the porte-cordon of Guillon; but the instrument about to be described is, in my opinion, superior in several respects to each and all of these. It was shown to me for the first time when in Paris in the summer of 1856, and was then, as I was informed, but recently introduced, and had been employed by several of the leading accoucheurs in that city. However, so far as I am aware, no description or notice of it is to be found in any of our recent obstetric treatises or medical periodicals; at any rate, it would appear, that it is not so generally known to the profession at home, as in my opinion it deserves to be; and for this reason I have been induced to call attention to it, and to offer a few descriptive remarks regarding it.

Description of the Instrument.—The instrument consists of two finely polished whale-bone rods, one of which is altogether straight, and set in a handle of the same material; the other terminates in a hook, and so adjusted by means of connective silver rings that it can with ease be made to glide parallel with or slide upon its fellow. The movements of the one rod upon the other is limited, by means of a stop, to about an inch, so as to prevent their separation when in use; the hook is intended for the funis, and the latter when received into it, can be securely retained by simply drawing down the rod of the same side, or pushing up the other, which comes practically to the same thing—the hook being for the time converted into a ring. The length of the instrument in my possession, including the handle, is $18\frac{1}{4}$ inches. The whale-bone rods, although somewhat longer, are not thicker than the tubes of Gooch's double canula, and will not occupy more space in the utero-vaginal canal than that instrument. The foregoing brief description will, I trust, suffice to indicate the principle, and convey a general idea of the mechanism of the instrument, whilst several other niceties of adjustment and minor details must, for want of space, be here omitted. The prolapsed portion of the cord when grasped

by the instrument, can easily be carried up or returned into the uterine cavity some distance beyond the presenting head, and be readily liberated or released by merely reversing the process or manœuvre necessary for seizing it. The cord will most probably gravitate out from the instrument the moment we wish to disconnect the two, and will fall upon, and obtain such support from some part of the child's body, so as to secure its permanent retention, and thus preclude the likelihood of its again descending. Any obstruction that the point of the instrument may meet with in its introduction into the uterine cavity, can be easily obviated by altering the direction of the instrument. The greatest obstacle likely to be experienced in its passage is a frequent recurrence of the pains, in which event we must relax or suspend altogether our attempts during their continuance. Before withdrawing the instrument from the uterine cavity, after the cord has been disengaged or set free, the rod with hook attached must be drawn down against its fellow, in order to prevent the hook catching upon, or getting entangled with, any part of the child, &c.

I am inclined to believe that this instrument is one which will commend itself to general acceptance, and that it will come ultimately to supplant and supersede the other mechanical expedients or instrumental methods hitherto employed for rectifying this procidentia of the funis. Looking to the small size, simple construction, easy application, and pliable and flexible properties of this instrument, I can conceive no instance of cord and head presentation where it may not be tried with much prospect of success, or where any injurious results would ensue when used with an ordinary degree of caution.

I have now had an opportunity of trying this instrument on two different occasions, and with a successful result in both instances. The following is a brief report of these cases as extracted from my note-book:—

Case I.—Prolapse of the Umbilical Funis before the head—Reduction successfully effected by the French instrument in the first stage of labour.

August, 1857.—Mrs. —, aged 31, primipara, had been about two hours in labour, and been attended by a student, by whom I was called to see her in consequence of the cord being in a prolapsed condition. I found the os uteri dilated to about the size of a florin, rigid and unyielding, and the liquor amnii for some time discharged. The head presented, and by the side of it a loop of the cord, about four inches in length, hanging into the vagina in an almost pulseless state. As turning and the forceps were, in the circumstances of the case, inadmissible, I resolved to attempt reposition of the funis by the instrument above described. The

instrument having been applied to the prolapsed cord, was, during an interval of pain, carried cautiously up through the os uteri, beyond the presenting head, till within a few inches of the fundus uteri. On the accession of the first pain the cord was easily liberated from the grasp of the instrument, and the latter was gently withdrawn. There was fortunately no recurrence of the prolapse. In the course of three hours afterwards the child was born in a semi-asphyxiated state, but after the use of the ordinary restorative means, it eventually did well. Mother made an excellent recovery.

Case II.—Descent of the Umbilical Cord by the side of the head. Reposition successfully accomplished by the French instrument during the first stage of labour.

February, 1859.—I was requested by a midwife to visit the subject of this case, in consequence of the cord having become prolapsed on the sudden discharge of the liquor amnii. The patient, a pluripara, had been in severe labour for upwards of four hours before my arrival. I found the os uteri unusually high up, and dilated to the size of half-a-crown. Towards the right side a coil of the cord about three inches long was prolapsed and distinctly pulsating. I attempted replacement of the funis by the fingers, and succeeded with some difficulty in pushing the cord up alongside the presenting head, beyond the influence of pressure, but in a few minutes it was again in the vagina as before. A second attempt was made, but with a like result. I at once procured the French instrument for returning the cord, and with it I carried the cord, during the absence of a pain, a short way beyond the middle of the uterine cavity. The instrument, with the cord still attached, was retained in this position till a pain came on, when the cord was disengaged, and the instrument cautiously removed. Labour terminated four hours afterwards without any subsequent descent of the cord. The child was lively and vigorous, and the mother made a good recovery.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

I. ON AMYLOID DEGENERATION.

1. *Archiv für Pathologische Anatomie und Physiologie, und für Klinische Medicin.* Herausgegeben von R. VIRCHOW. 1854-59.
2. *Die Cellular-pathologie in ihrer Begründung auf Physiologische und Pathologische Gewebelehre.* Von RUDOLF VIRCHOW. 1859.
3. *Journal de la Physiologie de l'homme et des animaux.* Publié sous la direction du Docteur E. BROWN-SEQUARD. Nos. V. VI. 1859.
4. *An Inquiry into the Existence of Amylaceous Compounds in the human body.* By Dr. BRISTOWE and Mr. ORD. Trans. Path. Soc. London, vol. x., 1859.
5. *British and Foreign Medico-Chirurgical Review.* October, 1860; January, 1861.
6. *On the Waxy or Amyloid form of Bright's Disease.* By Dr. T. GRAINGER STEWART. Edin. Med. Jour., February, 1861.

ANYTHING new in pathology, whether established in fact or simply ingenious in theory, attracts without fail the attention of the practical physician. The very awakening of this interest, however, is all but sure to call forth, among much that is characterized by talent and industry, writings full of crude ideas and questionable facts, which are apt to puzzle, if not to disgust, those who, less ambitious, study the matter purely with a view to their own information and improvement. The subject which we have selected for review is one of these, and our object in so bringing it before our readers, is simply to sketch such an outline as may enable those of them who have not dipped deeply into the matter to trace the successive steps by which Virchow has reached his conclusions, while we shall endeavour to indicate both what is clearly proved, and where evidence is still required.

The existence of *cellulose*, or of some member of this group of hydrocarbons, was unknown in the animal kingdom until 1845, when Carl Schmidt announced that he had discovered it in the Ascidians and Salpinæ.* This was amply confirmed by the researches of Kölliker, Löwig, Schacht, and many others. And subsequent observations rendered it evident that it was not confined to these alone. In the course of certain investigations into

* Zur vergleichenden Anatomie d. wirbellosen Thiere. 1845.

the structure and composition of the external skeleton of the Crustacea and the envelopes of the Tunicata, the organic bases of these textures were found to stand in close relation to *cellulose*, and were described accordingly as members of the *amylum* series, under the names of *chitine* and *tunicine*, to the chemical characters of which we shall have occasion to refer.

From a very early period in this investigation, the attention of Virchow had been strongly directed to this point, and the whole subject derived fresh interest in his eyes, as well as with other pathologists, from the discovery by M. Claude Bernard of a glycogenic function in the animal economy, which was then referred by that distinguished physiologist exclusively to the liver. M. Bernard's subsequent investigations, however, as well as those of M. Rouget and Dr. Pavy of Guy's Hospital, have shown that this view was erroneous, and that, instead of being confined to one organ, this function obtains throughout the whole, or nearly the whole, of the animal economy, and is due to the existence of what was called *glycogene* or *zoamyline*, a substance which is said to hold, in its chemical properties, an intermediate place between starch and dextrine. It is further characterized as follows:—

“When obtained pure, it is a whitish, tasteless, inodorous, neutral, non-crystalline powder, insoluble in ether, alcohol, caustic potash, and acetic acid, but soluble in water. It gives a rosy violet or bluish-red, and at times a chestnut-brown colour with iodine; it does not reduce the alkaline copper tests, nor enter into fermentation; but if boiled with dilute mineral acids, or placed in contact with saliva, blood, pancreatic juice, or diastase, it is converted into sugar, which ferments and reduces Barreswil's solution. When in a state of solution, alcohol and heat coagulate it, or precipitate it into a granular form.”*

The researches of Virchow to which we have alluded are extremely interesting, and afford a fair example of the indomitable energy and perseverance with which the learned professor, undoubtedly one of the first pathologists and microscopists of the age, tracks truth through the subtle windings and intricacies with which the path of the observer of nature is constantly beset, until he succeeds in placing his subject before the public with a logical clearness which we, in many cases, could scarcely have hoped for. He was first led to search in the direction here indicated by a resemblance which he detected between the cellulose tissue of the ascidians, which Schmidt had described, and the umbilical cord of man. This idea, as he tells us, being subsequently confirmed to some extent by the researches of Schacht, his observations were continued with increased energy. In many cases he failed entirely to discern any traces of the matter looked

* British and Foreign Med. Chir. Review. January, 1861.

for, until he directed his attention to the so called *corpora amylacea* of the brain.

Here he found—what he had all along been searching for—proof of the presence of *cellulose*, or some allied substance, in the human body. The test which he employed was that by which cellulose can be so easily detected in vegetable tissues, viz., iodine and sulphuric acid; and by means of it he was enabled to produce the characteristic reaction in the substance of the *ependyma ventriculorum*, and its prolongation into the substance which fills the canal of the spinal cord, and which has been described by Kölliker as the *substantia grisea centralis*. Virchow did not absolutely assert that this was cellulose, but only that the application of the test, with proper precautions, was followed by the beautiful violet colour which is known to be produced when the same reagents are applied to cellulose, from which he inferred the presence of an analogous principle.

Busk and Donders considered that the substance here discovered was actually starch. The former, in his remarks,* which are evidently based upon a painstaking investigation of the subject, says, “the corpuscles were starch and not cellulose, and possessed all the structural, chemical, and optical properties of starch, as it occurs in plants.” After describing the concentric lamination and general appearance of their structure, he goes on to state that the test afforded by polar light was distinctly observed by himself, and in this his observation is confirmed by Donders. Mr. Busk says—“Some of the smaller grains exhibited a distinct and sharply-defined black cross, of which the lines crossed at angles of 45° in the middle of the grain, but in the majority, there was only a single dark line in the long diameter of the grain, and which seemed always to correspond with an irregular fissure or hilum, as it might be termed, in the same direction, which was presented in a great many of the grains, and seemed to be the indication of a partial inrolling of them, as in the starch of the horse-chestnut. This longitudinal fissure was not unfrequently crossed by a shorter one at right angles.” Into this question we will not enter, although we may state that Dr. Bristowe and Mr. Ord assert in their paper, to which we shall have occasion again to refer, that the cerebral granules “presented no cross when examined by polarized light.”

The reaction by means of iodine and sulphuric acid is by no means one which is easy of application or unvarying in result, and this is a point which must be kept prominently in view in considering the discrepancies which obtain in the statements of different observers. These difficulties were admitted and clearly

* Trans. Microscopical Society. Vol. II., 1854.

stated at the very outset by Virchow, as the following extract will show. Referring to the reaction, he says—

“I have repeated this experiment so often, and with so many precautions, that I regard the result as quite certain. Not only have I instituted comparative researches in different human bodies, and in the most various localities, but I have also noticed the action of the reagents under all possible conditions. The experiment is best performed in the manner adopted by Mulder and Harting with regard to vegetable cellulose (vide Moleschott ‘*Physiologie des Stoffwechsels*,’ p. 103), viz., by causing the action of diluted sulphuric acid to follow that of a watery solution of iodine. The iodine solution should not be too strong, for the observation may then be impeded by its precipitation; and, on the other hand, care must be taken that the iodine exerts due action upon the substance. Owing to the volatility of the iodine, and its great affinity for animal substances, its action is usually very unequal, so that the border of the object and not the centre may be penetrated by it; or perhaps, of spots in close contiguity, one will contain iodine, and the other not. It is, consequently, always advisable to repeat the application of the iodine several times, but to avoid the addition of too much. Upon the subsequent addition of sulphuric acid, if the action has been too powerful, the result is a perfectly opaque red-brown colour. The most certain results are obtained if the sulphuric acid be allowed to act very slowly. In fact, I have procured the most beautiful objects in allowing a preparation covered with the glass to remain undisturbed, with a drop of sulphuric acid in contact with the edge of the covering-glass, for twelve to twenty-four hours. Under these circumstances, the most beautiful light violet-blue was occasionally presented. Lastly, I would just intimate that accidental mixture of starch or cellulose may readily happen, seeing that very light fibres or minute particles from the cloths with which the object and covering-glasses have been cleaned, may very easily be left upon them, which would afterwards exhibit the same reaction as the above.” *

With reference to the frequency of the accidental occurrence of starch and cellulose in microscopical investigations, noticed by Virchow in the last paragraph of this extract, we may here notice the confirmation which his views have received from the observations of M. Rouget, M. Balbiani, and Dr. Harris, who found starch grains in all conceivable and improbable situations, such as the hands, glass slides, windows, roofs, &c. “It is very common,” says M. Rouget, “to find them in suspension in the tincture of iodine.” This fact is further interesting as tending to explain certain curious statements on this subject which have appeared at various times.

Bodies, bearing a close relation to the *corpora amylacea* of the nervous system, are found in the prostate gland of adults, and the tissues about the neck of the bladder, where they may often be pressed from its follicles, and detached by means of washing. These, when small in size, and before they have undergone a process of calcification to which they are liable, may be shown to consist of granules, displaying a similar reaction with the same reagents. These were supposed by Wollaston and Robin,

* Archiv. B. VI. H. I. p. 135.

who probably examined them in the later stage of their metamorphosis, to be composed of lime and an azotized substance. Nägeli, on the other hand, has described them as absolutely identical with vegetable starch. We shall here, probably, as in most cases where extreme views are held, find the truth at some intermediate point, to be found in the masterly essays of Rouget* and Paulizky.† These writers, although they by no means agree in their conclusions, agree in referring the prostatic corpuscles unhesitatingly to the same class of compounds. While Paulizky considers them as very closely allied to starch, M. Rouget holds that they have certain characteristics of their own which serve to distinguish them from the other members of the same group. The fact probably is that the substance never exists in a state of purity, but mixed, or perhaps combined with some protein compound, in such proportion as in many cases to modify the reaction. There is at least one observation recorded which has an obvious and significant bearing on this point, which is to be found in the experiment of Lehmann, in which he succeeded in converting hæmatine (an azotized principle, not bearing the slightest analogy to cellulose) into sugar by means of nitric acid.

A knowledge of this will induce us to receive *cum grano salis* any evidence of the amylaceous nature of these corpuscles; at the same time, when taken in connection with the other proofs which we have indicated, the fact of Paulizky having succeeded both in exciting fermentation and in obtaining the well-known reaction of Trommer's test, seems to us to place the question almost beyond a doubt.

The discovery of these bodies in the brain and prostate, induced Virchow to continue his researches with unabated vigour and with remarkable results. Having now, we presume, acquired by long practice dexterity in the management of the iodine and sulphuric acid test, he investigated all the organs and tissues of the body both in health and disease. He soon discovered, in what has hitherto been known as waxy degeneration of the spleen, the Malpighian bodies transformed into small masses like boiled sago grains, which yielded, on the application of the above test, the characteristic violet reaction of starch.

From this discovery we may date a new era in the history of the subject. Hitherto it had only been demonstrated that there existed in the body certain principles, the presence of which had remained undetected up to that time, and which have not yet been shown to be abnormal elements of the tissues in which they

* Journal de la Physiologie de l'homme et des animaux. Tome II., Nos. v. and vi.

† Archiv. B. xvi.

have been found; whereas this new discovery of Virchow, upon which he has founded his theory of amyloid matter as a *materies morbi*, properly so called, induced him to apply to the tissues, so affected, the name of "cellulose degeneration." In the nervous system, liver, and kidneys, and latterly in almost every organ and tissue of the body except the osseous, his laborious investigations have enabled him to detect the same cellulose degeneration; and he has also recorded some cases of an unusually wide distribution of the disease. Other observers have succeeded in detecting cases of this kind, but the most remarkable example yet recorded was published by Friedreich and Kekule,* and occurred in a patient who had laboured for a year under tertian ague, and in whom the spleen was found to be twice its natural size, and of a waxy appearance, affording the usual reaction with iodine and sulphuric acid.

Under this name of amyloid degeneration we were somewhat astonished to recognize an old enemy long known to pathologists, and described under various designations. Thus we find that it is identical with the disease which we were taught by Dr. Gairdner, and other able pathologists of the Edinburgh school, to call "waxy" degeneration; and it is somewhat interesting to note that Dr. Gairdner in his investigation so far back as 1853, in which he was assisted by Dr. Sanders, came to the same conclusion as Virchow as to the tissues primarily affected. But while agreeing with Virchow that the small arteries and Malpighian tufts were first attacked, they came to a quite different conclusion as to the chemical composition of the morbid product. Dr. Gairdner describes this as "a peculiar modification of the protein compounds, in virtue of which they approached the character, and possibly the chemical constitution of keratin, and presented a much greater resistance than in the normal state to acid and alkaline solvents."†

Dr. Wilks, in an able communication in "Guy's Hospital Reports for 1856," on the pathology of what he calls "lardaceous disease," gives one of the best descriptions which we have seen of the physical characters of this disease. He evidently, however, considers it to be of a fatty nature; and in this he agrees to some extent with Meckel, who describes it as "cholesterine disease," believing that its properties were due to the presence of this fat. This view was, we think, most successfully refuted by Virchow who, in an elaborate paper,‡ showed clearly the differences which exist in their properties and reactions, between cellulose and cholesterine.

* Archiv. B. xvi. p. 50. 1859.

† Monthly Journal of Medical Science, vol. xviii.

‡ Archiv. B. vi. 1854.

He showed, among other proofs in refutation of Meckel's theory, that no fat will—1, become coloured with iodine alone; 2, remain colourless with sulphuric acid alone; and 3, afford a blue colour with iodine and sulphuric acid; and further, which is most significant, that in tissues in which cholesterine exists in abundance, as in the nervous tissue, none of the peculiar reactions of cellulose can be developed.

This amyloid degeneration, then, or by whatever other name we choose to call it, has received great attention from many able pathologists; but there has as yet been nothing like a satisfactory demonstration of the nature of the morbid matter. If, however, this hypothesis of Virchow be substantiated, the whole domain of pathology is revolutionized, and it, therefore, calls for the most careful and searching examination at the hands of pathological investigators. We shall not here, however, attempt more than a brief analysis of the proof *pro* and *con*, in so far as it has come under our notice.

The proof in favour of Virchow's hypothesis lies almost entirely in the production of the reactions which distinguish cellulose when brought into contact with iodine and sulphuric acid, and also with Schultz's solution (chloride of zinc and iodine). The so-called amyloid matter is no longer to be seen in the form of bodies closely resembling starch grains, and has not yet been found to be capable of transformation into sugar. That the reaction is actually obtained by Virchow we cannot permit ourselves to doubt, although we find in the papers of Dr. Bristowe and Mr. Ord statements which at first caused us to hesitate. Their conclusions, although arrived at only after patient and repeated investigation, are of a curiously negative nature. They were not only unable to discover the reactions described by Virchow, but were quite unable, as we stated above, to convince themselves of the accuracy of the observations of Busk, Donders, Rouget, and Carter, all of whom observed a black cross in the examination of the brain corpuscles by polar light, although there is some slight discrepancies in their statements as to its form. Now, proof of this kind is of an extremely unsatisfactory nature. We see no reason to doubt that these gentlemen failed in obtaining certain results, but we must object to attaching much importance to their statements, inasmuch as the fact of their not having seen so and so under the microscope is no proof in the world that Virchow, and those who have confirmed his observations, were equally unsuccessful; nor that those more skilled in the use of certain reagents may not have more satisfactory results. On the whole, we think that an unfair and undue importance has been given to the fact that certain observers have failed to produce a given effect, the production of which is attended with many difficulties, and requires great practice.

We observe that in almost all cases of failure with the reagents, the colour obtained is some shade of brown. Here, however, we have a result which Virchow has warned us against, as may be seen from a reference to the extract which we have cited above, where a reason for it is given. But this is not the only variation in colour against which we must guard. Besides mere variations in shade, Mr. Rouget directs attention to a bluish-green colour, which Virchow has attributed to a blending of the yellow colour, which is due to the presence of some azotized matter, with the blue of the amyloid substance. In short, it is evidently only in the most experienced hands that satisfactory results can be obtained.

The failure which has hitherto attended all attempts to reduce this substance into sugar, is a link in the chain of evidence on which Virchow's hypothesis depends, which we would fain see supplied; but we must not rashly conclude that because these attempts have failed up to this time, we may not hope to have, ere long, this missing link supplied. One fact which makes us rather sanguine on this point, is the great difficulty which was experienced in transforming chitine and tunicine into sugar. These substances, as we stated before, are to be found in the external skeleton of the crustacea and the envelopes of tunicata, and are admitted members of the starch group, intimately, and probably chemically, combined with some azotized matter. For a time that matter resisted all attempts which were made with a view to its conversion into sugar, until at last M. Bertholot succeeded, as M. Rouget tells us, "*par un tour de main particulier.*" M. Rouget has made some experiments on chitine, which are in the highest degree interesting, as illustrating the decompositions of such substances, and still further establishing their amyloid nature. M. Rouget writes:—

"By processes which I will indicate, and which are very analogous to those employed in obtaining the derivatives of cellulose, I have succeeded in modifying the properties of chitine, and in transforming it into a substance analogous to starch and dextrine, and finally in obtaining it in a state of solution, presenting all the essential characters of a solution of glycose.

"Fresh chitine boiled for about half an hour with five times its weight of caustic potash, which is moistened with water in such small quantity as to solidify almost instantly on cooling, gives off copious ammoniacal fumes, loses nearly half its weight (14 parts in 30) without the tissue losing any of its histological characters, and becomes colourless, transparent, gelatiniform, very friable in the wet state, very light, of an opaque white colour and, in the dry state, with a micaceous appearance.

"In this state the whole substance assumes a *violet* colour, by the action of the tincture of iodine alone, or with the aid of dilute acetic acid; and a *pure blue* colour by the action of the iodated chloride of zinc. It almost instantly dissolves in weak acetic and tartaric acids, and in water acidulated with 1-200th part of nitric or hydrochloric acids, especially on being gently warmed. It is precipitated in the form of thin starch or a semi-transparent jelly, by means of alcohol

or the alkalies. The dried precipitate forms a yellowish semi-transparent mass, which has all the appearance of dextrine or of gum. The solution in acids, and the precipitate obtained from these solutions, assume a violet-red colour by the addition of a few drops of a solution of iodated iodide of potassium (*iodure de potassium iodé*).

"Dissolved in concentrated sulphuric acid, the modified chitine colours the fluid yellow or brown; and if to the solution there be immediately added fifteen to twenty times its volume of water, it is deposited as a pulverulent whitish precipitate. But if this solution be left standing for from twelve to twenty-four hours, and if water be then added, the precipitate is much less bulky, and the liquor, neutralized by an alkali or by carbonate of lime, reduces energetically a solution of potassio-tartrate of copper, and becomes of a deep yellow colour when boiled with a concentrated solution of potash or soda—reactions which indicate the presence of glycosé. This modified chitine, soluble in acids, and the precipitate obtained from that solution by means of alcohol or alkalies, and susceptible of being dissolved in acidulated water, behave like homogenous substances. When treated, however, with caustic potash or soda lime, they continue to give off vapours which, when received on a slip of glass, moistened with hydrochloric acid, give rise to the formation of crystals of hydrochlorate of ammonia, the smallest trace of which can be recognized by means of the microscope."

It must not be forgotten, however, that the observation of Lehmann which we have mentioned, if accurate (and we must receive with the greatest respect any observation of his), would render such proof as we find in the above extract somewhat doubtful. But we think that his experiment, by transforming an azotized body directly into sugar, lends some confirmation to the theory advanced by M. Rouget that "amyloid bodies of animal origin have a particular constitution and special character, and that they establish a sort of transition between azotized substances on one hand, and what are properly called glycogenic substances on the other, between the immediate principles of animal formation and those of vegetable tissue."

The difficulties which must attend any attempt at ultimate analysis in the case of this amyloid matter are sufficiently obvious; but we are bound to state here that such attempts as have hitherto been made, have not only been unsuccessful in affording proof as to the hydrocarbonaceous nature of the substance, but have rather tended to prove, as we shall presently show, that the substance contained far too large a quantity of nitrogen to be reconcilable with the idea that any considerable portion of it was allied to the *amylum* series, or could be referred to any other of the hydrocarbons. The main difficulties in such an analysis, probably consist in the impossibility of obtaining the matter in a state of purity. Even in the case of the *corpora omylacea* of the brain, Schmidt was unable to demonstrate their hydrocarbonaceous nature, and the same observer was equally unsuccessful in his analysis of a waxy spleen.

By far the most satisfactory analysis on record, is that performed by Friedreich and Kekule in the case which we have

already noticed, and where the degeneration had gone to such an extent as almost to obliterate any trace of the original structure. After a careful proximate analysis of this substance, they proceeded to its ultimate analysis by selecting those portions of the mass which appeared to be the most completely degenerated. After extracting the soluble albumen, and the fatty matter and extractive, they found that the blue colour with sulphuric acid and iodine was still easily produced in the residual mass, although it was less permanent than before. Having then separated, as far as was practicable, the vascular fragments from the waxy matter, they submitted it to ultimate analysis with the chromate of lead. The result obtained bears the closest possible resemblance to the composition of pure albumen, as a reference to the following figures will show. The composition of the albumen which is given is the mean of five analyses by Dumas and Cahours, Lieberkühn, and Rüling.

	Albumen.	Amyloid Spleen.
Carbon,	= 53.54	53.58
Hydrogen,	= 7.16	7.00
Nitrogen,	= 15.66	15.04

Such an analysis as this, appears at first sight conclusive proof against the amyloid nature of the degeneration in question; but in weighing such evidence, it must not be forgotten that members of the starch group have been proved—1st, to exist in the animal kingdom; and 2nd, to contain nitrogen. The conclusion drawn by the analysts themselves was; “that waxy spleen contains no matter allied chemically with starch or cellulose.”

The relations which this form of degeneration bears to certain diseased conditions, have been variously noted by different authors. Thus Virchow remarks* that all the cases of waxy spleen he had met with were associated with disease of the osseous system, and principally with caries and necrosis. This, however, has not been confirmed by subsequent research, and it is now found to be of as frequent occurrence along with tuberculosis and syphilis, as with affections of the osseous tissue. Pagenstecher has collected from various authors 31 cases of this degeneration, and has found it to be associated in 10 cases, with disease of bone; in 1 with arthritis; in 4 with tubercle of the lungs; in 6 with marasmus and cachexia, without disease of the osseous system; in 8 with constitutional syphilis; and in 2 it was present in the kidneys without attendant Bright's disease. Dr. Wilks recounts 14 cases connected with caries and necrosis

* Archiv. B. viii. 1855.

of bone, 9 with syphilis and rheumatism, 6 with phthisis, and 6 with diseased spleen.

The existence of this affection as a form of Bright's disease, has quite recently been brought under our notice in a paper read by Dr. T. Grainger Stewart, before the Medico-chirurgical Society of Edinburgh.* The author has collected 20 cases; and, in the course of his very able paper, attempts to prove that the presence of this disease may be assumed during life, mainly by the increased quantity of urine and its diminished specific gravity. In an interesting discussion which followed the reading of Dr. Stewart's paper, and in which Drs. Gairdner, Bennett, and Haldane took part, these gentlemen refused to admit the term "amyloid degeneration" until the evidence on the subject was more complete; and preferred to retain the term "waxy" which originated in the Edinburgh, as did "lardaceous" in the Vienna school. Dr. Gairdner called attention to the errors which had been promulgated, and too hastily adopted with reference relative to the "fatty kidney," which was then stated to be not only a form of Bright's disease, but the very essence of the disease itself. There could be no doubt now, he said, that this was a very enormous error, and he therefore cautioned the society against the danger of hasty generalizations.

Dr. Stewart's paper merits a careful perusal, while the author deserves no little credit for what is, we believe, his first contribution to medical literature. The symptoms indicative of an accumulation of amyloid matter in the kidneys, are stated by him as follows:—

"An individual who has long suffered from wasting disease, such as scrofula, caries, necrosis, or syphilis; or who, though without palpable disease, is of a feeble constitution, feels an increasing weakness, and begins to pass large quantities of urine, and to drink largely. He is, contrary to his usual custom, obliged to rise repeatedly during the night to make water, and on each occasion passes a considerable quantity. The amount of urine varies from 50 to upwards of 200 ounces daily, always bearing a relation to the amount of fluid drunk, generally nearly equalling it in amount, or sometimes even exceeding it. The feet and ankles become oedematous after a hard day's work, but return to their natural condition during the night's repose. In many cases there is observed a hardness and swelling in the hepatic and splenic regions, dependent upon an increase of bulk of the liver and spleen. Patient feels a constant lassitude and unfitness for exertion. His urine gradually becomes albuminous, and a few waxy or hyaline tube-casts are to be found in the very scanty sediment which it throws down. It is of low specific gravity, 1005 to 1015. The patient may continue in this state for months, or even years; may, indeed, undergo a temporary improvement, the liver and spleen becoming diminished in bulk; but, sooner or later, for the most part ascites and general dropsy gradually supervene, accompanied frequently by diarrhoea, which is at times found quite uncontrollable. The urine, now very albuminous, diminishes in quantity, so as at times to be almost or altogether suppressed; effusions into the serous cavities,

* Edinburgh Medical Journal. February, 1861.

or severe bronchitis, ensue; the patient becomes exhausted, and sinks; or drowsiness comes on, and the disease terminates fatally amid coma and convulsions."

The author adds a more detailed examination of the symptoms thus briefly sketched, making reference as he does so to various pathological states, of which they are presumably the expressions. The patient begins, as one of the earliest symptoms of his malady, to pass large quantities of urine of low specific gravity; "the amount of urine passed being even sometimes greater than the ascertainable fluids consumed during twenty-four hours." This abundant secretion is not at first albuminous, but presently becomes so; the albumen increases in quantity; hyaline tube-casts are thrown off; and as the albumen increases, the amount of fluid passed diminishes. The preliminary increase in the flow of urine in amyloid degeneration Dr. Stewart lays claim to having first observed; and he lays much stress upon it as a diagnostic symptom. He refers it to an increased transudation from the bloodvessels, consequent on loss of controlling power in their degenerated muscular fibres. He refers the subsequent decrease in quality to the blocking up of the *tubuli uriniferi* with tube-casts and epithelium, which often exhibit cells with fatty degeneration from prolonged functional inactivity; and partly to a progressive diminution of calibre in the vessels as they become more extensively affected. That the albumen should get more abundant as the vessels become more diseased, is only natural. Examination of the hyaline tube-casts is unfortunately not so useful in enabling us to make a diagnosis as might at first sight seem probable; for, in the first place, fatty epithelial cells are not unusual in the amyloid form; and, in the second, the amyloid degeneration seems rarely to extend to the epithelium of the tubules—never, in the experience of Dr. Stewart himself—so as to enable an observer to obtain any reaction from elements imbedded in the coagulated casts. Diarrhoea is a frequent and formidable symptom in the later stages of amyloid disease; it is attributed with much probability, and with the sanction of Virchow, to a sometimes almost universal degeneration of the smaller arteries in the intestinal mucous tract. Since the arteries of the whole alimentary canal have been found involved, from the mouth to the anus, the occasional occurrence of vomiting and hæmoptysis, if this last be not due to a complication with pulmonary disease, may sometimes be referable to the same condition of the vessels. The state of the kidneys and other abdominal viscera brings on dropsy towards the close, and often severe bronchitis; dyspnoea from the latter cause sometimes terminating the patient's life. Of course, in the event of a flux through other channels, more especially a diarrhoea, supervening, as in some recorded

cases at an early stage of the disease, the characteristic diuresis might very likely be absent.

The author gives the following recapitulation of the leading symptoms of the disease :—

“Amyloid, or waxy degeneration, is associated with a cachectic appearance ; with an increased flow of urine, which, later in the disease, becomes albuminous, and contains tube-casts, generally hyaline. Urine diminishes towards the end of the case, and dropsy then gradually supervenes. There is frequent diarrhoea, hepatic and splenic enlargement. The patient dies either from exhaustion or from uremic poisoning.”

To recapitulate ; we assume it as proved that certain principles have been discovered in the human body, as well as in the lower grade of the animal kingdom, which are closely analogous chemically to the starch or cellulose of the vegetable world ; but whether these, as found in the brain and prostate, are actually morbid products properly so called, or not, is a question still somewhat doubtful. As for the degeneration which is now termed “amyloid,” we cannot but consider that its amyloid nature is far from conclusively established. There are, as we have endeavoured to show, certain facts connected with its proximate and ultimate analysis which tend to throw the strongest doubt on the accuracy of Virchow’s hypothesis ; but we must not forget the difficulties which have been overcome in the case of other animal substances of a nature which is considered to be similar.

The results obtained by Virchow, however, and by those who, like him, have attained to dexterity in the use of the iodine and sulphuric acid, as well as the whole history of the origin and progress of the subject since the discoveries of Carl Schmidt, induce us, although far from convinced as to the accuracy of the hypothesis in question, to consider that Virchow has established a *prima facie* case. The subject is, however, so enveloped in obscurity, and its investigation has hitherto been attended with such difficulty, that we must wait for farther proof before we can agree with Professor Virchow in his conclusions.

As for the name itself, we have no objection to its adoption. Of the former designations “waxy degeneration” was certainly the least objectionable, and we would not in the present state of the subject advocate any change, were we not under the impression that the term “amyloid degeneration” is in some respects a better one. The terms “waxy” and “lardaceous” depend on the physical character alone of the disease, or, as Dr. Grainger Stewart says, “not upon the essential disease, but upon a result of it,” while the term “cholesterine disease” has no claim upon our attention. But the name adopted by Virchow appears to us to be the most correct of the whole, provided we, in adopting it,

limit its signification to "a degeneration which offers some of the chemical characters of amyloid matter." For the rest we can well understand how our Edinburgh friends cling to the name which is connected with their own school. With it the names of several of them have been long and honourably connected, and we sincerely trust, that if further investigation results in the superseding of the term "waxy degeneration," it will never be forgotten how much we owe to Drs. Gairdner, Bennett, and Sanders, in relation to the pathology of this disease.

II.—ON CONGENITAL MALFORMATION OF THE TESTICLE.

- 1.—*Etudes sur la Monorchidie et la Cryptorchidie chez l'Homme.*
Paris, 1857. Pp. 180, roy. 8vo.
- 2.—*Recherches Tératologiques sur l'appareil Séminal de l'Homme.*
Par Ernest Godart, M.D., &c., &c. Paris, 1860, roy.
8vo. 14 lith. plates. Pp. 148.

Of the first of the above works we have only seen an abstract. From this it appears that the author gives an account of the condition depending upon the non-descent of the testis on one or both sides, from the history of previous authors, and from cases coming under his own observation.

The more important conclusions which he has deduced from his study and observation are the following:—

M. Godart defines monorchidia to be the absence of one of the testes, from its not having descended from the abdomen into the scrotum, and cryptorchidia to be the absence of both testicles from the same cause. These affections are both to be distinguished from congenital anorchidia, which is the term applied to the entire absence of one or both of the glands, and of which several cases are described by the author in both of the works above referred to.

The causes of monorchidia he states to be either—1. Hereditary predisposition; 2. An arrest of development of the corresponding side of the body; 3. An inflammation of the testis; and 4. Malposition in the course of the descent. M. Godart describes, step by step, the descent of the testis, and informs us that lesions of the gubernaculum testis are more frequently than is generally known the cause of malposition of the organ. The left testicle he states to be more frequently arrested in its passage than the right.

Although the arrested testicle is of an apparently perfectly healthy glandular structure, and secretes a fluid resembling semen, yet in none of the cases which have come under the author's observation has the fluid been found to contain spermatozoa. But if the second testis has descended into the scrotum, it alone is sufficient for fecundation.

The arrested testicle may be the seat of morbid alterations of various kinds, such as tubercle, cancer, &c., and M. Godart states that these diseases are most liable to occur in the arrested organ of the right side.

It is more rare to find the descended testis diseased and the arrested one healthy. M. Godart describes such cases in which the morbid affection was various, and states, as the result of his observation of four cases, that such persons are capable of ejaculation (from the secreted product of the arrested testicle as he supposes), but the fluid discharged contains no spermatozoa.

M. Godart has observed only three cases of monorchidia, in which both the descended and the arrested testis were affected with disease.

Cryptorchidia.—This condition, which is a malformation in man, is the natural condition in a number of animals, in which it interferes in no degree with the generative powers.

He regards the causes of this condition the same as those of monorchidia. He describes several cases, and states as the usual modification of the system accompanying it a moderate stature, fair complexion, little vigour, high voice, youthful appearance, timidity, &c.

Persons afflicted with cryptorchidia in whom the testes are well developed are capable of ejaculation, but the semen is without spermatozoa, and they are incapable of fecundating. Four married persons in this condition whom he had observed had had no children.

In addition to the facts and views now stated the work appears to contain a number of interesting minor contributions to the history of the functions of the productive organs of generation.

The second work is in part a reproduction and extension of the author's inaugural thesis, sustained at the Medical Faculty of Paris in 1858, "On the Congenital Absence of the Testicle." It contains, in addition to the general history of his subject, an account of the author's observation of a variety of interesting cases of malformation of the seminal organs, and is illustrated by numerous well-drawn delineations of the external appearance and deeper dissections of these cases.

After some preliminary and general considerations upon the physiological relations of the various kinds of glands, the author

describes a number of cases of unilateral anorchidia, or absence of the testicle.

In these cases it appeared that there existed very various degrees of absence of the conducting tubes, but in all of them the body of the testis was entirely absent. In some the epididymis, vas deferens, and vesicula seminalis existed, and in various degrees; in others one or other of these parts, or all of them, were also absent. When the vesicula seminalis and vas deferens existed, they contained a considerable quantity of their usual secretion, but without any spermatozoa being detectable in this fluid. Such cases are to be distinguished from those which are the result of castration by the existence of a cicatrix in the latter, and from those which are the effect of injury by the presence of the testicle, though reduced in size in the latter instances.

In such cases, if the other testis is sound, the man does not appear to suffer from the congenital absence or imperfection of the organ on one side, but on the contrary he possesses all the powers which belong to those possessing both organs perfect. If, however, the testicle which is present is affected with disease, or even if it should not have descended into the scrotum, the person will be incapable of procreation. Ejaculation may be possible, but it will be without effect.

In cases of the congenital absence of both testicles, a very different state of matters exists. The external organs are little developed, scarcely passing in size those of a boy of ten or twelve years of age. The hairs on the pubis are in general scanty and soft, and the intra-pelvic portion of the organs presents the same inferiority in its development. The scrotum is also absent in those cases in which the epididymis and vasa deferentia do not exist. The person so affected is entirely impotent and incapable of ejaculating any seminal fluid; but this is to be distinguished from the case of double cryptorchidia, or testes arrested in their descent, in which the individuals are capable of seminal discharge, but incapable of procreation, as there is an absence of spermatozoa in the fluid ejaculated.

Individuals affected with double anorchidia are usually weak and timid, exhibiting little mental capacity, of a soft habit of body, and having a shrill high voice. They are, in fact, in all respects similar to those who have been mutilated in early infancy.

The third class of cases described by M. Godart, presents considerable interest—consisting of those in which the one or both testes have been present, but with an imperfect or closed condition, or more or less complete absence of the vasa deferentia and vesiculæ seminales.

In such cases the vasa deferentia may be defective in some part of their course through the inguinal canal, or they may be wanting at either terminal portion. In the case of the terminal portion being wanting, the vesicula seminalis is also absent; and in the case of the commencement being defective, the epididymis generally participates more or less in the same imperfection. But it is a remarkable fact that in several of these cases the testis was of considerable size, of natural glandular structure, and appeared to have secreted a fluid resembling semen, although spermatozoa were not, in several instances, detected in it.

In some instances it is to be remarked the vesicula seminalis is absent, while the vas deferens is natural.

The most remarkable fact, however, upon which the author insists, is that in some of these cases, notwithstanding the absence of a considerable part of the emitting tubes, the testis was not only of natural size and structure, but had actually secreted a fluid containing the natural spermatozoa. But this state of matters does not appear to have induced any unnatural condition of the gland, or of the system in general.

With reference to the influence which the absence of the seminal passages may exercise upon the generative function, M. Godart refers to the experiments of Sir Astley Cooper, Mr. Curling, and M. Gosselin,* as well as some of his own, in proof of the fact that the removal of a part of the seminal ducts, which is always followed by closure of the divided ends, does not induce atrophy of the testis, but that on the contrary the gland continues to be developed in young animals, and maintains nearly its natural size in adults, and besides appears to continue to secrete natural spermatozoa. Of course the animal is rendered sterile, as the semen cannot be excreted by the natural passages; and it is equally surprising in those animals experimented on, and in the cases of congenital closure of the passages, that the distension of the parts of the ducts remaining in connection with the testis does not appear to produce any serious hurt to the organ, nor general inconvenience. We may suppose that the secreted fluid is removed by absorption into the system.

In such persons it would appear that the venereal appetite may exist, and that ejaculation of a fluid may be possible; but it need scarcely be added that if the affection exist on both sides, they must be entirely sterile.

* *Nouvelles Etudes sur l'Oblitération des voies Spermatiques et sur la Sterilité consécutive à l'Epididymite bilatérale.* 1848.

III. *The Elements of Agricultural Chemistry.* By THOMAS ANDERSON, M.D., F.R.S.E., F.C.S., Professor of Chemistry in the University of Glasgow, and Chemist to the Highland and Agricultural Society of Scotland.

ALTHOUGH the little manual before us refers to a department of chemical science not immediately bearing upon our profession or its practice, there are two reasons which induce us to notice it here. We find the first of these in the fact that the author is so intimately connected with the Glasgow medical school, while the second is derived from a perusal of the work itself, in which the author displays a special talent in the communicating of scientific knowledge in a form which must prove attractive, not to scientific readers alone, but also to those who are supposed to be possessed of elementary knowledge only. We do not, however, mean to say that the work is what is usually called a popular one, or one in which facts are just seasoned with sufficient science to make it palatable to the dilettante reader. On the contrary the author has most wisely devoted a large portion of his treatise to the exposition of the laws which regulate the growth and metamorphoses of animal and vegetable tissues, and to the organic and proximate constituents of these tissues, before turning to the practical part of his subject; thus affording to the intelligent farmer a fund of knowledge, by which he can not only appreciate the practical details in which he is so deeply interested, but by means of which he may be enabled to develop original ideas, which again he has abundant opportunities of putting to a practical test.

The application of chemical science to agricultural pursuits has made vast progress since the publication, in 1840, of Liebig's celebrated treatise, "Chemistry, in its application to Agriculture and Physiology." In earlier times, although the phenomena of vegetation were assumed to depend on certain chemical changes occurring in the plant, most erroneous ideas were held on the manner in which these changes were brought about. Thus we find Van Helmont attributing the growth of plants exclusively to water, while subsequent writers referred it in like manner to the atmosphere. Jethro Tull refuted both of these hypotheses with success, but unfortunately substituted a theory of his own, which was even more objectionable than the other two, seeing that they were partly true, whereas his was absolutely false; he held that the plant absorbed and digested the soil itself in minute particles, and founded a peculiar system of husbandry on this assumption. Towards the end of last century, the flood of light

which the master mind of Lavoisier threw on chemistry in general penetrated the veil of obscurity which had hitherto hung at the very threshold of agricultural research, and paved the way for brilliant discoveries in this, as in all the other branches of chemical science. Soon afterwards an able treatise by Lord Dundonald, father of the late naval hero of this name—"On the Intimate Connection between Chemistry and Agriculture"—appeared, which was immediately followed by a work of still greater ability and deeper research, from the pen of De Saussure. Of these two works the former appealed more directly to the farmer, but neither of them succeeded in furthering in any marked degree the progress of agriculture. In 1812 Sir Humphry Davy's lectures, founded on the experiments of De Saussure, attracted great notice, and it was now thought that the attention of the farmer had been permanently arrested. This, however, was but temporary, and it was reserved for Liebig, in the treatise above mentioned, to establish the chemistry of agriculture on a solid and enduring basis.

Unfortunately, however, the farmers were led to found on this the most extravagant hopes and the wildest delusions, while the utter failure of these, foretold by the whole chemical world, dashed for a time the hopes of those who trusted that science was now in the ascendant. But for a time only. The great truths had taken hold of many minds, and, during the past twenty years, their number has increased a thousandfold, while their progress has been adorned with a series of most brilliant discoveries and stupendous practical results. Such is agricultural chemistry at the present day, the subject of the work before us.

It is not our intention to go deeply into a consideration either of the subject or of the book itself, but we will confine ourselves to a brief sketch of its plan and contents. After devoting several chapters at the commencement of the work to a consideration of the organic, inorganic, and proximate constituents of plants, as well as the changes which take place in their food during their growth; the author treats of the soil at some length, both as regards its general characters, and the improvements of which it is susceptible by mechanical means, such as draining, subsoil and deep ploughing, &c. The general principles of manuring, as well as a thorough consideration of the various manures, occupy, as might be anticipated, a very considerable portion of the work. In one of the chapters we find the composition and properties of farm-yard and liquid manures noticed at some length. This is a point where clear comprehension is of paramount importance to the farmer, inasmuch as the management and disposal of these matters is under his immediate control, and all scientific facts which will enable him to do this economi-

cally and to the greatest advantage, have an obvious practical bearing on the subject.

The author, in the chapter on the valuation of manures, shows how it is that this, which is in many respects a commercial rather than a chemical question, comes to be treated of by chemists. He clearly points out the system of valuation now generally adopted and the calculations on which it is based, by means of which the farmer can, provided he knows the exact composition of the manure, determine with the greatest ease its exact commercial and agricultural value. The laws by which the rotation of crops is regulated are now clearly and concisely set forth, and the author concludes with a chapter on the feeding of farm stock, in which many valuable practical directions are given; while it is in every case distinctly explained on what scientific grounds such and such a procedure is adopted in preference to another.

Dr. Anderson's work, although avowedly an elementary one, is certainly one of the best, if not the best, we have seen on the subject of which it treats. It cannot, we think, fail to be of great use to the farmer, both by enabling him to give true scientific reasons for what he does, and in showing how far he may expect from science assistance which may be turned to a practical use, as well as—what is evidently no less the object of the author—to indicate as clearly as may be where possibility or probability in science ends; all which, when properly understood, will doubtless enable the intelligent agriculturist to apply all the resources at his command to the best advantage, with a view to the greatest possible pecuniary results. We confess to having experienced no little pleasure and derived no little information from a perusal of this manual, which contains much that is interesting both to the physiologist and to the medical man.

IV.—*Traité des Maladies Mentales*, par le Docteur B. A. MOREL,
Médecin en chef de l'asile des aliénés de Saint-Yon, &c., &c.

THE present century was far advanced before the enlightened views, which now regulate the management of the insane, were carried out in all our asylums. It is surprising that, with the advances which took place in almost every branch of medicine, the important subject of mental diseases, which was so well treated by some of the ancient medical writers, should have been neglected for so long a period. Not only did the subject make no advance, but it actually retrograded, and, from the time of Cælius Aurelianus until our own days, a space of nearly 1800

years, the treatment of the insane was shrouded in a mass of ignorance and superstition; the unfortunate victims of it were exposed to cruelty and neglect, such as to make us shudder when we think of it; and the wonder with which we contemplate how recently this state of matters existed, is only equalled by the heartfelt gratitude with which we view their ameliorated condition.

The interest that has attached to this branch of medicine, and the advances that have been made of late years, may be inferred from the numerous works which have appeared on the subject, either as monographs, or on limited portions of psychological medicine. These are of great value to the specialist physician, but they are not so well adapted to the use of the general practitioner; and, as it is now expected of every well-educated medical man to have a knowledge of the nature and treatment of insanity, a systematic treatise coming from the hands of a writer so well known as M. Morel, who has devoted so much attention to the subject, must be admitted to be of great value. For the use of the student and general practitioner it is more particularly intended, as we gather from the preface, as well as from the general plan and scope of the work.

It is divided into four books, the first being devoted to an exposition of the doctrines held on insanity among the ancient physicians, during the middle ages, and at present; the second to the causes and the manner of interpreting them; the third to the symptomatology; and the fourth to the particular forms of insanity and their treatment.

It is not within the limits of our present intention to enter on a complete analysis of these; indeed, to do justice to the book, extending as it does to upwards of 800 pages, and containing the results of much observation, a very lengthened notice would be required. We intend, therefore, to draw attention almost exclusively to what appears to us most to demand it, viz., the new classification of mental maladies which the writer employs; and, as this is based on the causes of insanity, it will be well at the outset to glance at their arrangement.

They are divided into—

1. General predisposing causes, as civilization, religious, political, and other influences.
2. Individual predisposing causes, as hereditary predisposition, education, temperament, &c.
3. Diseases of the circulatory, respiratory, and digestive systems, as causes of insanity.
4. Physiological causes, as menstruation, pregnancy, &c.
5. Specific causes, as intoxicating substances, &c.
6. Moral causes.

In a former work by the same author, on the physical, intellectual, and moral degeneration of the human species, he enunciated the opinion, that amongst the circumstances which act upon and modify the human race, one set is not opposed to the maintenance of health nor the perpetuity of the species, while another class leads, by a more or less prolonged action, to such a degradation that normal life is no longer possible, and that in the course of some generations their reproduction must cease. "Such," says he, "is the predominant idea of the treatise on degeneration, which has led me to create different varieties of noxious influences, which, under the effect of the same degenerating causes, put on invariable pathognomonic characters of the intellectual, physical, and moral orders. These are what I call the fixed, unalterable, essential characters, which ought to serve as the groundwork of a classification of mental diseases."

This, as M. Buchez remarks, is evidently a new method of considering the effect of hereditary predisposition in diseases, no one having asserted that certain affections, certain intoxicating substances, certain habits in the parents, had the power of creating in their children a consecutive condition, transmissible even to the extinction of the race. Hereditary taint was only recognized in the light of an affection analogous to that which had been observed in previous generations.

Pursuing this idea, M. Morel was led to conclude that the best means to obtain a classification corresponding with the general nosological arrangement, was to adopt one "essentially based on the etiological element, and thus to avoid the too exclusive method hitherto followed in classing the insane according to the disturbances or lesions of the intellectual and affective faculties."

In the eighth chapter of the second book we find the arrangement on which M. Morel has decided, and we give his observations in his own words:—

"Our etiological researches have sufficiently demonstrated that, if the predisposing and determining causes do not produce the same results in every individual, it is nevertheless impossible to deny their effects in those who, under the influence of those causes, become insane.

"We have seen how, in a multitude of circumstances, affections such as phthisis, meningitis, typhoid fever, pneumonia, hypertrophy of the heart, &c.; how moral impressions, such as fear, love, exaltation of religious sentiment—how all the passions, in a word, may cause insanity. But it does not necessarily follow that all who are subjected to these causes become insane, or indeed are threatened with insanity. The reason is plain. The causes which we have designated generally under the name of determining or exciting, do not provoke insanity in a necessary or absolute manner. The causes called specific, certain idiopathic affections of the cerebrum, such as that which produces progressive paralysis, have alone the power of necessarily producing permanent disturbance of the intellectual faculties in certain circumstances. . . . Although I have

expressed in a formula the law of an intimate relation necessary between the form of insanity and the nature of the cause, I do not mean that this relation is established under the influence of the slightest cause which, in generating the elements of unsoundness, may lead ultimately to insanity. To my mind, the cause can be separated neither from the predisposition of the individual nor the functional disturbance, or lesion, which this cause inflicts on the organism.

"To the idea of classification which I entertain, three elements are necessary for the production of a particular form of insanity—predisposition; an exciting cause; and functional disturbance or lesion.

"If predisposition does not exist, the exciting cause may indeed produce disturbance of the intellectual functions, general or systematic, durable or transitory aberration, in a word insanity; but it will rarely have the character of that which we observe in the essential forms of the disease.

"If the cause leave but a passing impression on the organism, the unsoundness which may be the consequence is transitory. When it is permanent, it becomes the index of insanity, or in other words, of that state which in a psychological point of view, is not only the expression of general disturbance in the system, but of this or that disturbance corresponding with the nature of the cause, and with that of the lesion. It is in this light only that I say, that intimate and necessary relations are established between the nature of the cause and the form of the disease.

"If it were otherwise, if the different forms of insanity became organized in an invariable manner under the influence of the slightest determining or exciting causes, these forms would be innumerable, and any attempt at classification would become impossible, in presence of that host of causes which would have the fatal privilege of creating forms of insanity, corresponding to the specific nature of their action.

"In that case it would only be necessary to designate the form of insanity under the name of its determining or exciting cause, and we see at once into how many errors of diagnosis and prognosis we should be led. Does not experience prove to us every day, that we are induced almost invariably to make the final play the part of the determining cause, and so to neglect the real point of departure of the malady, or in other words, of that cause which gives to insanity its particular form, which prevents our confounding one variety with another, and the insane of one class with those of another?"

Such are some of the general remarks with which the writer prefaces his classification. He closes them thus:—

"It is on these principles," says he, "that I now attempt to give a classification of mental diseases, in which these affections are not classed according to exaltation or depression, such as we see in Mania, Melancholia, &c. These phenomena are amongst the symptoms found in every variety of insanity. My aim is to discover in each form, characters distinguishing it from others; so that, these fundamental characters being given, we may recognize the nosological variety to which an individual belongs, who reveals his condition by the unsoundness of his ideas, by his acts, and by the different lesions or disturbances of the nervous functions."

The following is the classification he adopts:—

1. Hereditary Insanity.
2. Insanity from Intoxication.
3. Insanity from the transformation of certain Neuroses.
4. Idiopathic Insanity.
5. Sympathetic Insanity.
6. Dementia.

In the first group are contained those who are endowed by inheritance with a nervous temperament, and readily become the subjects of insanity, monomaniacs, cases of moral insanity, imbeciles and idiots.

The second group includes not only those who are addicted to habits of intoxication by alcohol, &c., but likewise those who have suffered from insufficient or improper nourishment, who have been exposed to marshy influences, or some peculiar geological constitution of the soil, cretinism for example.

The neuroses alluded to in the third group are hysteria, hypochondria, and epilepsy.

The fourth—idiopathic insanity—comprises cases of progressive weakening or obliteration of the intellectual faculties, as the sequel of chronic diseases of the brain or its envelopes, general paralysis, &c.

The fifth group, sympathetic insanity, includes erotomania, nymphomania, &c.

Dementia forms the sixth, and M. Morel remarks that he considers it useful to preserve this as a separate division, although dementia is not a primitive but rather a terminative form; and, as very many cases of insanity end in this way, and are all recognizable by common characters, he does not think that the method of classification which he endeavours to introduce will suffer therefrom.

The author concludes his remarks on classification in these words :—

“ I shall no doubt be reproached for having omitted two classes of the insane generally adopted—mania and melancholia; but I have already said that mania (exaltation) and melancholia (depression) are symptoms which we meet with in every variety of insanity, and which do not constitute essential forms of it. Besides, I do not deny the value of these designations, and think they ought to be retained. The expressions maniacal excitement, melancholic depression, mania, and melancholy, are employed frequently by me when I desire to describe the phases of any of the forms of insanity belonging to my classification, but these symptoms are only transitory phenomena. When I employ them I do not wish to indicate particular forms of the disease, but the symptoms of a certain form of alienation. . . . In this as in all the sciences of observation, the results are sufficient to justify the method. If then this classification of diseases of the mind, assist us in understanding better the course of these very insidious maladies, and gives a more solid groundwork to the prognosis and diagnosis, I shall believe that I have attained the very difficult aim which I proposed to myself.”

We think great credit is due to M. Morel for the ingenuity of his classification, and that he has made a valuable addition to the literature of insanity; but we fear that the difficulty of practically applying his method will be very great. Although he thinks that in every insane person signs and symptoms are to be found which point with certainty to the origin of the disease,

we believe that great doubt would overhang many cases, and render it no easy matter to decide under which category they should be placed. We do not think that in our present imperfect knowledge of the mind in health and disease, we can hope to attain to more than an orderly arrangement, such as is necessary in a systematic consideration of the subject. If we could ascertain the powers of the mind implanted in it by nature, or if we knew the physiology of the brain, a table of mental affections might be formed according to the modifications in the former, or the changes in the structure of the latter; but as we have said that is not compatible with the present state of the science.

In taking leave of the work before us, although we may not agree with the writer in thinking that a perfect nosological table can be constructed on the ethiology of the disease, nevertheless much may be said in favour of the attempt, and we think that both on this question and throughout his work, M. Morel has displayed a desire to respond to the scientific aspirations of the age of progress in which we live.

V.—*Transactions of the National Association for the Promotion of Social Science*, pp. 900. 1860. London: John W. Parker, Son, and Bourne, West Strand. 1861.

WHATEVER difference of opinion may exist as to the amount of practical good likely to result from the labours of the Association for the Promotion of Social Science, there can be none as to the laudable intention of its founders. Nor can it be questioned that hitherto it has been a great popular success. It furnishes a platform, easily accessible, for the promulgation of opinion and the ventilation of philanthropic projects. The subjects comprised within the scope of its programme are innumerable, while the literary and mental requirements of its essayists are not necessarily great. This state of matters, however, cannot in some points of view be regarded as favourable to its continuance or influence. As long as the novelty lasts, and as long as great intellects and great names grace by their presence and direction, so long probably will the association prosper. But we are afraid people will tire of the constant reiteration of platitudes, and the ever-recurring enunciation of hobbies and crotchets which must of necessity form no inconsiderable portion of the transactions.

The last meeting of the Association was, as our readers will recollect, held in this city in the month of September. No fewer than 2872 tickets were issued, and 164 papers were read. The

department allotted to public health was that of course which chiefly engrossed the attention of the medical profession; and we doubt whether in any of the other sections the essays were of the same practical importance and general interest. We must, however, except from commendation the very verbose address of Mr. Chadwick, which fully merited the impatient reception it met with. Mr. Chadwick is one of those men who has devoted a lifetime to the red-tapeism of sanitary science, and who, in certain circles, is accepted as a high authority. That he is a laborious student cannot be denied, but he seems to accept opinions as facts, and to take his evidence from all accessible quarters without being willing or able to estimate its value. His address on public health appears to have been put together without method or arrangement—to be defective in all the characters which such a general *resumé* ought to present, and to place in too prominent relief the labours and plans of its author.

Dr. Wallace of Greenock contributes an admirable paper on some of the causes of the high rate of mortality in Greenock. His conclusions as to the remedies to be adopted to meet the physical causes of this excessive mortality are well worthy of reproduction:—

"1. That it is desirable that landowners in towns, or the vicinity of towns, and selling land for building purposes, should have it surveyed so that (a) the streets should not be below a fixed minimum width, say fifty or sixty feet; (b) that the intervals between any two streets should not be below a fixed minimum width; and (c) that the area to be built on should be clearly defined, and the building of back lands entirely prevented.

"2. That in all towns there should be a court in which the feu contracts should be registered, and without the permission of which no new buildings, or alterations of old ones should be made.

"3. That the local authorities in towns should have the water supply in their own hands, and that they should have power to compel every feuair to lead branch pipes into his property.

"4. That the inspector of nuisances should not be removable by the local authority, but by some neutral party, such as the sheriff or board of supervision.

"5. That house proprietors shall provide their tenants with conveniences in the shape of proper ash-pits and water-closets.

"6. That in towns the streets, as a general rule, should be causewayed, and have side pavements of flat hewn stones, the expense to be borne by the existing or subsequent feuairs on the line of the streets.

"7. That all streets shall have main drains, the expense to be borne in the same way as that of the causeway.

"8. That each tenement should have a tributary drain leading into the main one.

"9. That the surface gutters and drains, particularly where the water supply is abundant, should be frequently flushed, more especially in warm weather.

"10. That for the effecting of such improvements as the widening of streets, the local authorities should have the power to purchase property, the price to be paid to be determined by mutual agreement, or failing that, by a reference to a judicial authority, whose decision should be final and binding."

Dr. Wallace regards want of personal and domestic cleanliness, and ignorance in the training of children, more especially as regards clothing, as fruitful sources of the high mortality prevalent in Greenock. To these he adds disregard of medical aid, which is manifested in three ways — (a) By not procuring it; (b) by delaying to procure it; (c) by refusing or distrusting it when actually procured. In 1859 no fewer than 25 per cent. of the deaths (excluding sudden deaths) were entered by the registrar as having no medical attendance. Of those who do seek aid many delay till too late; others frequently change their doctor. We commend Dr. Wallace's paper to the careful perusal of all interested in the health of large towns.

Of the papers which treat of infantile mortality we defer consideration, as the subject is partly discussed in this number, and will probably soon again come under our notice.

Sir Charles Hastings enters the lists as a determined opponent of tobacco-smoking. The new crusade which has been commenced against this practice is not likely to meet with much success; and, in common with all similar movements, the object in view is rendered more unattainable by the extreme language used and the dogmatic assertions made.

Dr. Macleod contributes a very excellent paper on "Sanitaria for the Sick Poor of Glasgow." We agree entirely with him as to the desirability and advantages of such institutions. He is of opinion that cottages of wood or iron would be well adapted for the purpose; that suitable sites could be easily procured on the Clyde; and that the expenses might be defrayed, partly by voluntary contributions, and partly by payments from those of the artisan class who availed themselves of them. Such institutions have been established elsewhere and met with success, and it can easily be conceived what a boon they would be to convalescents. In all philanthropic schemes of this kind, there are great preliminary difficulties to be encountered, but with zeal and energy these might be overcome.

Among other topics of general interest discussed at the association was the Eastern or Turkish bath, on which Mr. Erasmus Wilson furnished a paper. Some years have now elapsed since a Turkish bath was established in Dublin, but it is only recently that the example has been followed in Glasgow. Any one who knows the craving of the public for novelty, and its credulity on sanitary or medical matters, will not be surprised to learn that so far it has been a great success. In fact for the last six months the Turkish bath has been the popular remedy for every ailment. If one complains to a friend of being ill or out of sorts, he is sure to be advised to try the Turkish bath. If for amusement he reads the *Field* newspaper, he will find the Turkish bath occupying

the leading columns, to the exclusion of the usual piscatorial or hunting intelligence. We are glad to say that this importation has met with but scanty favour from the heads of the profession, and that a note of warning has been sounded by the medical press of the country. Mr. Wilson considers it as a preservative of health, preventive of disease, and curative of disease; but these conclusions are not deduced from any sound data. Admirably adapted as the Turkish bath may be for some climates and some races, we have no hesitation in saying that its indiscriminate use in this country is sure to be attended by pernicious effects. One case of death shortly after taking the bath has already occurred in this neighbourhood, and elsewhere dangerous consequences have been traced to its influence. In a very short time, like other fashions, the Turkish bath will succumb to common sense.

Dr. M'Ghie in a paper on "Accidents: their causes and modes of prevention," states that since 1846 there have been on an average 512 cases admitted into the Glasgow Infirmary. Of the whole number 1 in 9·7 died; and the female mortality exceeded the male by nearly 1 per cent. As regards external causes, the most numerous class of cases is that of falls from heights, 1 in 7 having occurred from this cause. Dr. M'Ghie concludes his interesting paper by affirming "that a great proportion of these accidents are preventible; it would perhaps be going too far to say that the whole of them could be prevented. If every member of society were more careful and attentive to his own safety and the safety of others, than he is ever likely to be, such catastrophes as we daily witness in such a city as this, would be rarer in their occurrence than they now are. In speaking of the causes of accidents, if we say that any particular accident was caused by a fall from a height, or by a fall on the street, we only explain the external circumstances through which the event occurred; but if we inquire more minutely, we shall probably find that in one case out of every two or three, the person so injured was intoxicated more or less, and that the intoxication was the direct cause of the accident."

We have merely alluded to the two or three papers above mentioned, to indicate the nature of the subjects treated of in the public health department of the Social Science meeting. The reader of the *Transactions* will find many others of great importance; and in some of those in the section of social economy, topics in which all medical practitioners are interested are discussed. Whatever be the future fate of the association, the volume for 1860 will at all events always be a standard memorial of its visit to Glasgow.

VI.—*A Practical Treatise on the Ætiology, Pathology, and Treatment of the Congenital Malformations of the Rectum and Anus.* By WILLIAM BODENHAMER, M.D. New York : Samuel S. and William Wood. 1860.

ELABORATE works on special points in medicine and surgery are often of great practical value. But to make such a work really valuable, there is required not only research, industry, and experience on the part of the author, but also a certain fitness in the subject chosen, without which labour is expended in vain. It is truly matter of regret that so many works issue from the press which have caused to their authors such a vast expenditure of mental labour and midnight oil—an expenditure far beyond what the subject required, or even merited. But yet, regret it as we may, no season passes without some such work being brought under our notice, and it is by no means a pleasant task for the reviewer to talk slightly of a work like that before us, in regard to which, although it bears the undoubted stamp of both industry and talent, the error we have referred to has obviously been committed. It is far from our intention to assert that the subject of “congenital malformation of the rectum and anus” is not an important one, but we do deny that it is of an importance sufficient to warrant Dr. Bodenhamer in squandering the talents which he undoubtedly possesses in a wild-goose chase after literary fame. It is common enough for an author who has devoted much attention to a given subject to assume that his readers must needs think of it with the same exaggerated interest which he himself feels, but yet no error can be more fatal to literary success. Sure are we, that the author who, in writing, loses sight of his readers, is as sure to fail as a player who, forgetful of his audience, would mouth and rant for his own amusement. That Dr. Bodenhamer has so exaggerated the importance of his subject will, we think, be made evident by a perusal of the subjoined extract from his preface; while this will also serve to show to some extent the nature and scope of the work before us:—

“Whatever may be said with regard to either the merits or the demerits of this work, all will admit that the subject of it is of the highest importance, and that such a production as is here aimed at, is decidedly a desideratum in surgical literature. The utility of such a treatise is undoubted, and will not be called in question.

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“Notwithstanding several very able and highly valuable contributions on this subject have been made within the last half century, nothing like a general treatise has been attempted. Of the principal contributors to this literature in later years, the names of Benjamin Bell, Thomas Copeland, and A. Copeland Hutchison of England; MM. Amussat and Roux de Brignoles of France; Von

Ammon and Friedberg of Germany; and Dr. Bushe, Dr. Barton, and Dr. Gay of the United States, may be mentioned as the most important. "The Boston Society for Medical Improvement" has within the last few years highly distinguished itself by investigating this subject quite thoroughly, and Dr. Gay, already named, one of its able members, has produced a valuable paper on it. It is, however, more especially to the genius and master pen of M. Amussat of France that we are indebted for having drawn from oblivion, as it were, this long neglected and forgotten subject, and for restoring it to that rank to which it is entitled.

"Although these eminent surgeons have by their able productions inaugurated a new surgical epoch with regard to the anatomy, the physiology, and the treatment of these congenital vices of conformation, yet it is surprising the little advancement which is being made on the subject. A congenital imperforation of the anus and rectum is still regarded by many surgeons merely as an anatomical *lusus naturee*, beyond the power of art to remedy, and is still to a great extent the *opprobrium medicum*.

"The investigation of these abnormal conditions of the rectum and anus is invested with a deep interest, not only as an important pathological inquiry, but above all as conducive to the adoption of measures calculated to be highly beneficial to a class of little sufferers, the most unfortunate and deplorable. The subject is a very important one, and is presented not for the benefit merely of the curious in anatomical pursuits, but on account of its practical relations, and for the ultimate and permanent good of the little sufferers themselves; consequently it presents itself to the humane surgeon as an object demanding his most deliberate and serious attention, for among the many forms of death which surround the cradle and which are the objects of parental care and solicitude, those which depend upon a malformation, or an imperfect state of the excretory passages, are perhaps the most distressing in their nature, and make the most painful impressions upon the minds of the parents.

"In the elucidation of this subject, the author disclaims all pretensions to an extensive practical and personal knowledge of it; indeed this seldom falls to the lot of any one practitioner, however extensive his practice may be.

"It may be alleged that in treating this subject, the author has been guilty of great repetition and prolixity, but as the subject is so important, and has been so little discussed, he thought it might prove beneficial to enter into it somewhat in detail.

"Whether this work will prove to be valuable or not, it has been the result of much reflection and long labour, and the author claims for it entire originality in the general design and treatment of the subject. The extent of his researches will be understood when it is known that he has consulted and cited the large number of authorities presented in the Bibliography at the commencement of the Introduction. This extensive Bibliographical Index forms of itself a complete history of these congenital vices, and will greatly facilitate the study of them.

"In this work the practitioner will find reported nearly three hundred cases, collected from the numerous sources, being by far the largest number ever presented in any one single production on the subject. The record of these cases will show their singular variety; they have all been carefully classed according to their species, and most all of them have been reported in full, and as near as could be, in the precise language of their authors. Many of these reports have been translated from the French, the German, and the Latin, especially for this work. The author has made but few remarks in relation to any of them, preferring to present in full the instances themselves, as facts, from which each practitioner might form his own opinion, and draw his own conclusions. It will be seen that the observations of authors and the cases, have been generally collected from the original sources, instead of, as is often the case, from the mere references made to them by others."

Dr. Bodenhamer has divided his subject into nine varieties, each treating of a distinct or pseudo-distinct form of malformation. The following are the varieties :—

"First Species.—This species consists of a preternatural narrowing of the anus at its margin, and occasionally extending a short distance above this point.

"Second Species.—In this species there is a complete occlusion of the anal aperture by a simple membrane; or by the common integument, or a substance analogous to it, more or less thick and hard.

"Third Species.—In this species there is no anus whatever, the rectum being partially deficient and terminating in a cul-de-sac at a greater or less distance above its natural outlet, without any communication whatever, either externally or internally.

"Fourth Species.—The anus in this species is normal, but the rectum at variable distances above it, is either deficient, obliterated, or completely obstructed by a membranous septum.

"Fifth Species.—In this species the rectum terminates externally by an abnormal anus, located in some unnatural situation, as at some point in the sacral region; or the rectum is prolonged in the form of a fistulous sinus and terminates by an abnormal anus, at the glans penis, the labia pudendi, or at different points in the perinæum. The natural anus being generally absent, its functions are performed by the abnormal one.

"Sixth Species.—The rectum in this species opens preternaturally into the bladder, the urethra, or the vagina; or into a cloaca in the perinæum with the urethra and the vagina. In these instances the normal anus does not generally exist.

"Seventh Species.—In this species the rectum is normal, with the exception that either the ureters, the vagina or the uterus, open preternaturally into it.

"Eighth Species.—In this species the rectum is entirely wanting.

"Ninth Species.—In this species the rectum and the colon are both absent, and there is usually an abnormal anus situated in some extraordinary part of the body."

After, in each case, minutely describing the anatomical peculiarities and the mode of physical exploration, he gives the diagnosis and prognosis, and passes on to a most elaborate notice of the treatment requisite. He then gives a number of cases in illustration, many really valuable, but most of them so short in their details as to be perfectly useless to the surgeon who seeks for practical information, while, as a whole, their compilation must have been a labour which we shudder to think of. In our opinion, Dr. Bodenhamer would have done far better had he reduced the size of his book by one-half, which he might have done by giving those cases only in detail which were of real interest and practical importance; while cases like the following, which constitute the vast majority of his 287 cases, would have been far more usefully set forth in a tabular form specifying, for example, the mode of operation and result. In the latter case they might have given results which would guide the practitioner to the mode of operation to be chosen, and enable him to give his prognosis with more confidence. Indeed, we do not think we would exaggerate did we aver that no man on earth,

save the author and the printer, would ever read the cases as they now stand. What can he expect us to learn from a string of cases like the following?—

"Case CCXXX.—Henkel relates a case in which no rectum existed. (*Mem. Med. Chirurg. Anmerkungen*, 11. 1772.)

"Case CCXXXI.—Esterio mentions a case in which the rectum was absent. (*Instit. Chirurg. tome 11. Sec. v. cap. 163. No. 1.*)

"Case CCXXXII.—Bonn reports an instance in which there was a complete absence of the rectum. (Papendorf. *Opus. citatum.*)

"Case CCXXXIII.—Ludovicus gives an instance of a child in whom no rectum existed. (*Miscellanea curiosa sive ephem. acad. natur. curiosor. ann. III. decur. i. observ. 257.*)

"Case CCXXXIV.—Huber reports a case in which the rectum was wanting. (*Acta Physico-Medica. tome vii. Observ. 24. p. 64.*)

"Case CCXXXV.—Matani cites the case of a child in whom the rectum was wanting. (*Orteshi Giornal di Medicini. tome iii. p. 250. Padova.*)

"Case CCXL.—Giering mentions an instance of a child in whom no rectum existed. (*Sel. Med. Francof. tome iv. p. 137.*)

"Case CCXLI.—Fitteau reports the case of an infant in whom the rectum was entirely absent. (Sedillot, *Recueil Periodique. tome ii. p. 101.*)

"Case CCXLII.—Carvenon cites the case of a child in whom the rectum was wanting. (Sedillot, *Recueil Periodique tome ii. p. 36.*)

"Case CCXLIII.—Oosterdyke reports the case of an infant in whom no rectum existed. (Papendorf. *Opus citatum.*)

"Case CCXLV.—Otto cites the case of an infant in whom the rectum was absent. (*Pathologische Anatomie. Breslau, 1813.*)"

But we would be very unjust to Dr. Bodenhamer did we not frankly acknowledge that there is much in his work both of interest and merit. In the first place, there are many cases of great interest which his industry has here placed in juxtaposition; and secondly, there is much which merits praise both in style and matter. The following is a good description of the general symptoms:—

"Should any of these congenital vices of structure have unfortunately escaped the observation of the accoucheur or the nurse at the time of the birth of the child, its existence in the majority of instances would sooner or later manifest itself by a train of morbid phenomena simulating strangulated hernia, the result of the retention of the meconium and other matter. If no alvine dejections take place within twelve or twenty-four hours after birth, the child gradually becomes restless, and by its peculiar plaintive cries manifests the suffering it now begins to endure. These cries are generally attributed by the nurse to colic, and the little sufferer is treated accordingly with all kinds of medicines, but generally to none but the worst of purpose. The abdomen, especially in the hypogastric region, now becomes enlarged, tense, hot, shining, and painful upon pressure, the respiration becomes difficult and irregular, and the pulse frequent, small, and contracted. To these symptoms, if no amendment soon takes place, vomiting will be added, first, of all the milk and other fluids swallowed, then of the mucous and biliary secretions, and finally, of the meconium, or a dark brownish matter analogous to it. Should no relief still be afforded the little sufferer, these symptoms will become augmented in violence; the diaphragm and other abdominal muscles will become excited to violent expulsive efforts, during which respiration will sometimes become suspended, the face will

become swelled, discoloured, and covered with perspiration; the voice sooner or later will become almost extinct; there will be hiccup, with coldness and flexure of the extremities, and convulsions. In the male, inflation of the scrotum and penis sooner or later takes place. Should matters thus continue, death is inevitable and is soon ushered in; and it usually takes place between the third and the eighth day, according to the vigor of the little patient. Before death occurs there is often a general yellowness of the skin. When the case is protracted for a number of days the emaciation becomes extreme, and the patient dies from the effects of inanition.

"Strange to say, that cases of complete occlusion of the rectum have occurred, in which life had been prolonged for a number of days, and even for months without any evacuation from the bowels, and before any violent symptoms had taken place."

With this extract we must conclude, but not without noticing the beautiful plates, sixteen in number, with which the book is illustrated. These form, in our opinion, by far the most valuable part of the book. The anatomical relations and surgical management of the parts are here delineated with a rare fidelity and distinctness which is really instructive; while the description of each plate enables us to form a fair idea of the subject treated of by the author, without much reference to the body of the work. Dr. Bodenhamer merits praise, which we have freely accorded to him, for his industry and perseverance, but we must repeat our expression of regret that his work is not one which is likely to be of use to the surgical world at large. It certainly exhausts the subject, and may possibly be useful as a work of reference in a doubtful case, but as a useful and practical addition to surgical literature, it is, in our opinion, and for the reasons above stated, a complete failure.

VII.—*The Medical Critic and Psychological Journal*, edited by FORBES WINSLOW, M.D., D.C.L., Oxon. No. I. January, 1861.

The Cornhill Magazine, No. XV. March, 1861.

THOUGH not yet classed among the medical periodicals of the day, the "Cornhill" promises to be more medical than some of its professed medical contemporaries. Omitting the poetical effusions, of eight prose articles, four are medical; of these two are stories, in which the "sayings and doings" of medical men form the principal part of their contents, and two are essays on "Health" and the "Turkish Bath."

The "Medical Critic and Psychological Journal" is our old friend the "Journal of Psychological Medicine and Mental Pathology" under a new title, and with more extended pages. The present is the third transformation which this periodical has undergone, and we have no doubt that the more varied and less

professional character of its contents will secure for it a more extended circle of readers. It promises to provide its readers with "a series of essays similar in literary character to the leading articles in the *Times* and *Saturday Review*, illustrative of the present and prospective condition of the medical profession in its moral, social, political, literary, and scientific relations." One half of its pages are to be devoted to psychological subjects, and the other half to articles bearing upon general medical politics, literature, and science. Questions and books are to be reviewed in a fair, impartial, and liberal spirit, apart from feelings of a personal, private, or party character. It is to be feared that, if the leading articles of the *Times* and the papers in the *Saturday Review* are to be the literary models of one half of the contributions to this journal, the fairness, impartiality, and liberality of spirit which are promised will not be their distinguishing characteristics. There may be smart and vigorous writing—there may be caustic wit and scalping criticism, but if the models selected for imitation are faithfully followed, there will be found in them little evidence of those excellent qualities which the introductory "notice" would lead us to expect. It is pretty generally admitted that pictures, books, or periodicals which are merely copies or imitations, usually fall far short of the celebrated originals which they profess to imitate. Their defects and blemishes are more likely to be exaggerated in the copy, while the beauties and excellencies of the original are defaced. It would, we humbly think, have been more consonant with good taste and sound judgment, if the distinguished editor of the "Medical Critic" had omitted to state in the way that he has done, what was to be the future character of his wares.

We shall now briefly glance at the contents of the first number of the new and improved "Psychological." We have first of all the "Quarterly Retrospect," the epithet "psychological," which was formerly applied to this portion of the journal, being omitted. The character of the materials of which the "Retrospect" consists is a sufficient warrant for this omission. Psychology—a term of sufficiently definite import in works on mental philosophy—has, we humbly think, been prostituted by the editor of this journal and others to uses which it was never meant to serve. Everything connected with lunatics, laws of lunacy, lunatic asylums, lunatic trials, and a thousand other things with the most remote bearings upon "minds diseased," are all classed under the expansive title of psychology. It is time that this abuse should be corrected, and that things lunatic should be called lunatic. Fine names don't alter the nature of things, and it is a safe rule on all occasions to call a spade a spade.

When we state that the "Retrospect" consists of rather large

miscellaneous clippings from the *Daily Telegraph* and *Times* on such subjects as the "Tobacco Controversy," "Book-hawking," "Labourers' Cottages," "Drainage," &c., and from the "Addresses at the Social Science Congress," held at Glasgow in the "terennial quarter of the year," it will be sufficiently evident that it is more distinguished for the variety than for the congruity and interest of its contents.

Of the thirteen articles five are on lunatic subjects, and the remaining eight gravitate between "The Marvellous" and "Medical Gossip," and include such topics as "Diphtheria," "The Syllogism," "Specialism," "The Wear and Tear of Medical Life," "Maternal Love in Nature," "Reason, Genius, and Madness," with a well-merited threnody, entitled "In Memoriam," on the late Dr. Todd.

Of the lunatic papers, "Criminal Lunatics" is a review of Dr. Hood's letter to the chairman of the commissioners in lunacy, and the popular version of it which appeared in the first No. of *Temple Bar*. Both the literary style and opinions of Dr. Hood are treated in a somewhat contemptuous and satirical manner; but the conclusions of the reviewer leave the subject of how this troublesome class of cases should be best dealt with pretty much *in statu quo*.

On turning with some interest to the paper entitled "Medico-Legal Studies on the perversion of the Moral and Affective Faculties in the precursory period of General Paralysis," we were disappointed to find that it was merely a translation of the paper by M. Brierre de Boismont which appeared last year in the *Annales d'Hygiène*.

Translations of papers from other journals not having been mentioned in the "notice," are we to accept them as an instalment of the series of essays, similar in character to the leading articles of the *Times*, and papers in the *Saturday Review*?

"Maternal Love in Nature" is, we find, also a translation from the original Dutch of a popular lecture by Schroeder van der Kolk to the Physical Society of Utrecht, at one of the meetings to which ladies are admitted! The remarkable announcement of the translator—who says of the lecture, "It appears to me that the distinguished author has, in this essay, with peculiar force and beauty, shown, that in the works of nature the great truth that 'God is love,' is exhibited in a manner second only to its demonstration in the all-important fact, declared by the Saviour himself, that 'God so loved the world, that he gave his only begotten Son, that whosoever believeth in him should not perish but have everlasting life'" —precludes the possibility of criticism.

"Reason, Genius, and Madness" is the enticing title of a review of a controversy between two French savans, MM.

Moreau and Flourens, on the interesting question whether or not genius is "a morbid affection of the nervous system—a *neurosis*?" M. Moreau who, though an "alienist," does not, we presume, consider himself a lunatic, and consequently does not pretend to be a genius, says Aye to this proposition, and M. Flourens, who defends his order, says No.

"The Marvellous" is M. Figuier's designation of what is better known in this country as "spiritualism," or as some call it "spiritism." This article is the continuation from the former series of an elaborate refutation of the insane outpourings of the "Spiritual Magazine," and the *Rev. T. L. Harris*, and the *jeu d'esprit* of Mr. Thackeray's friend of twenty-five years' standing. The manner in which the subject is handled in this paper, is fitted to do more harm than good, giving to it, as is done here, an importance which it does not merit. It could not be left in better hands than those of Mr. Punch, who, in such cases, wields with so much effect the weapons of ridicule and caricature.

"Specialists and Specialities" may be rendered syllogistically, after the manner of the paper which precedes it:—

First, as a universal affirmative,

All specialists are good;

is not true.

Second, as a particular affirmative,

Some specialists are good;

is true.

Third, as a universal negative,

No specialist is good;

is manifestly untrue.

And fourth, as a particular negative, we may safely and logically affirm that

Some specialists are not good.

We might next treat the subject by conversion, and affirm that

Some good men are specialists;

and

Some good men are not specialists;

which are both quite true.

We might also handle it by contraries, subcontraries, and contradictories; and after exhausting the whole battery of logical formulas, we would conclude with the proposition with which we started.—Q. E. D.

"The Wear and Tear of Medical Life" is a melancholy picture

of the troubles and afflictions of medical life in London. It is written by a hand who might distinguish himself in the pages of the *Saturday Review*. We trust, however, for the sake of our unfortunate brethren in the metropolis, that the colour of the picture is overcharged, and that the drawing is not perfect. In no point of view can the fate of the metropolitan practitioner, whether he be of high or low degree, be made tolerable. If he does not succeed, he becomes seedy, "clad in rusty black;" if he succeeds, he is worked to death. "Each healthy old man who has weathered the storm, represents how many who have been wrecked, stranded, or gone adrift?"

The following words of warning may be of use to the youthful aspirant after practice:—

"For the most part, young medical men commit a great mistake on first starting. They overhouse themselves. Their establishment eats them up. They have not the courage to live in a small way. Then they marry too early. It is a virtue on their part, and speaks volumes in their favour. But house expenses are certain, and professional returns uncertain. Children must be fed: servants must be paid. The wife likes the habits of a lady, decorates her rooms, and receives her friends, while her husband lives in the streets, that he may sleep in a mansion. For a mansion it is to him, that vast pile of bricks and mortar, for which he pays an enormous rental, and which he has furnished beyond his need. For the sweets of life are unknown to him. He toils only for his daily bread. He has no time for visiting. He can never receive properly. His occupations unfit him for doing so. His real post, and the one in which he shines, are the sick chamber of the wealthy and the hovels of the poor. Beyond their precincts he is nobody. How can it be otherwise? If he is a man of fashion, he is unfit for his profession; or, if he be a philosopher, he is unfit for fashion. Add to these eccentricities and burdens an equipage—a shining, well-turned-out equipage! This is the climax of folly that has brought many a general practitioner to the ground. It is time enough to ride in a carriage when you have realized capital, or should be lucky enough to possess some private means of your own. Only, private means alters the question, which, as we are now looking at it, is one of pure, unaided, professional ability.

"It has often occurred to us, that most medical men would be the better if they remained single. We know that it is opposed to the received opinion on the subject, and we own that it has its inconveniencies. But we feel confident that, in the present state of society, in which expensive luxury forms a constant element, it is next to impossible for a general practitioner to support a proper appearance in the world from nothing more than the proceeds of his professional exertions. It is the married life that urges so many to work themselves to death. They cannot bear to see their family less than they should be. Consequently they are ever on the fret. They have no leisure to sit down and think. They cannot and must not do so; and it is owing to the cares of matrimony that many, who would otherwise have been philosophers, devoted to their profession, end by becoming nothing better than routiners or professional tradesmen. In moments of real illness and danger, the public do not ask whether the doctor rides or walks, is married or unmarried. All they require is that he should be at hand when he is wanted, and should be capable of performing all that is required of him."

SELECTIONS FROM MEDICAL JOURNALS.

I.—ON APOPLECTIFORM CEREBRAL CONGESTION IN ITS
RELATION TO EPILEPSY. By PROFESSOR TROUSSEAU.

THE following paper was read by M. Trousseau before the Académie de Médecine. It has excited in Paris, and elsewhere, an interest almost unprecedented. Besides many communications in the medical journals, it has provoked a discussion which has occupied the greater part of nine or ten *séances* of the académie, and in which MM. Bouillaud, Piorry, Larrey, and many of the most eminent men in Paris took part. M. Trousseau said:—

There is, as you are aware, an opinion in the scientific world that *apoplectiform cerebral congestion* is a common malady. This is a thing so well established and so universally accepted that one cannot appear to doubt it but with a bad grace. During a two years' residence as *interne* in the lunatic asylum of Charenton, I saw, or believed that I saw, a considerable number of apoplectiform congestions; since that time I believed still that I saw a certain number, both in hospital and in private practice; but during the last fifteen years I have seen none. And yet my brethren see quite as many as before. It follows then that either I or they must be deceived, and it is equally clear that I believe the fault to be on their side; otherwise I would change my opinion.

To begin, then; a man, with or without premonitory symptoms, is suddenly struck down with apoplexy. He is raised stupified, and for a quarter of an hour, an hour, or longer, he remains with the head heavy, the intellect confused, the gait unsteady. The next day it is all over.

This person is said to have had apoplectiform cerebral congestion. Once I said so too, but I say so no longer.

Another suddenly, in walking, has an attack of vertigo; he loses his sight and speech, utters a few unintelligible words, staggers, and sometimes falls, raising himself mechanically, these symptoms sometimes lasting for a few seconds. There is now only a feeling of weight in the head, sometimes a momentary overclouding of the intellect, and in three or four minutes order is restored.

This one is said to have had slight cerebral congestion. Once I said so too, but I say so no longer.

Why then, gentlemen, have I changed my ideas? It is certainly through no love of paradox; it is because facts have thrown a new light on my mind.

A friend of mine, in 1845, was found unconscious in bed. His face was turgid and purplish; intelligence, as well as motion and sensibility, was gone, and there was stertor. This was a strong man, forty-two years of age. His wife did not know how long he had been in that state, but had sent for me at once on being awakened by a strange snoring. Even then I had given up bleeding in apoplexy, and so, placing him in a half sitting position, I gently struck his face with a handkerchief moistened with cold water, applied two ligatures at the upper part of the thighs in order temporarily to retain a large quantity of venous blood in the inferior extremities, and waited the result. Scarcely an hour had passed before motion was restored and sensibility re-established; and he then replied with some coherence to questions put to him. The following day nothing remained but a great feeling of weariness.

Some time after this I was sent for in haste to visit one of my neighbours, seventy years of age, who while in the boulevard had been struck with apoplexy. He had remained fully a quarter of an hour without assistance, and, when I arrived, consciousness was returning. He did not, however, recognize me; looked about with a stupified look, and moved his legs and arms, without being conscious of what he did. His lips and nose were swollen and his eyes injected. Gradually, and in a few hours, he recovered without any active treatment. His valet told me that he had had several attacks of a like nature during the past two or three years, and that the effects had gone off in the same manner, once after bleeding, and on the other occasions after a foot bath with mustard.

In the same year I was consulted by a provincial advocate, thirty-five years of age, who had had in six months three attacks of apoplexy. He congratulated himself that he had been bled on each occasion: he had been purged, and every month had three leeches applied to the anus. The last attack had taken place while he was going up stairs to his house after an important pleading; the head had struck on the steps of the stair, and he still bore the mark of a rather deep wound on the forehead. His intellect, as well as sensation and motion, were perfect when I saw him, and the consequence of the attack had lasted not more than an hour.

I have difficulty in believing in apoplexies in persons of thirty-five years, especially when the attacks are repeated every two months; and immediately the idea of epilepsy presented itself to my mind, and I told my fears to the medical man who sent him to me. He replied that nothing justified my conclusion, and that convulsions had never been seen. I held, however, to my diagnosis, and in a very short time, in full court, the poor advocate was seized with a violent attack of epilepsy, which unfortunately left do doubt in the mind of any one, and he was obliged to quit his profession.

My attention was now awakened, and I asked myself if many of the people I had seen with apoplectiform cerebral congestion were not epileptic; and I kept myself on my guard. My first patient soon had other attacks, and he has now sometimes as many as four or five epileptic seizures daily, with very frequent attacks of *petit-mal*; his sight is lost and his intellect seriously impaired. As for the old man, an account of whose case I have briefly given, he is still alive, and almost every year has one or two of these attacks. Since his fall on the boulevard he never goes out without a servant, and the latter told me that while his master is lying on the ground his face is contorted, while one arm is affected by spasmodic contractions, which last but for a minute, but which suffice amply to characterize epilepsy.

Since that time, whenever I have been consulted with reference to an individual with apoplectiform cerebral congestion, I have examined with the greatest care whether from time to time during the day there were not sudden and transient giddiness with the characteristics indicated above; whether these attacks of congestion were not rather nocturnal than diurnal; whether they were not accompanied at their commencement with *nervous* movement, and almost always, when the seizure has taken place in the presence of witnesses, convulsions could be recognized. When the congestion had taken place at night, during sleep, I learned sometimes that the urine had escaped involuntarily, and that, for some days the tongue had been painful, while the face, forehead, and neck had been covered with small ecchymotic stains, like fleabites. Above all I learned that the attacks took place at no long intervals, leaving no lasting effects. In a word, epilepsy became evident, when looked for with a wish to find it. No month passes without my seeing in my consulting-room persons supposed to be subject to apoplexy, who are epileptics.

There is perhaps scarcely a week that I am not consulted by adults, old men, or children, who, having been attacked with epileptic vertigo, have applied to me as having slight cerebral congestions. And although epilepsy in all its

forms is better known to-day than it was twenty-five or thirty years ago, many medical men, nevertheless, refuse to believe in the existence of so terrible a disease; and if they recognize it, they are unwilling to tell the family what they believe, and prefer to leave to us that sad mission. Very frequently epileptic vertigo reveals itself by symptoms always attributed to cerebral congestion, and to which practitioners who practise the treatment of the insane, have long since directed the attention of their brethren. After the attack, it is common enough to see the patients become delirious for some minutes, and the delirium may even continue for a considerable time. Judicial annals and the police records are full of suicides and murders too often attributed by the medical men to what they call cerebral congestion, whereas, they must be attributed to epilepsy. We may say, almost without fear of error, that if a man, without any premonitory intellectual derangement, without having hitherto given any sign of insanity or of mania, without being poisoned by alcohol or any other substance which exercises a powerful action on the nervous system, commits suicide or kills some one—we may say that that man is an epileptic, that he has had a severe fit, or, as is more frequent, an attack of epileptic vertigo. These strange acts are, I repeat, attributed by most medical men to passing cerebral congestion, because the regular fit is sometimes misunderstood, and the vertigo almost always.

Another cause which frequently induces mistakes in the diagnosis of epilepsy is, the repugnance which families have in revealing, even to medical men, this sad malady. Even when a mother has witnessed a fit, she refuses to believe in epilepsy, and if she be questioned by the medical attendants, she speaks of coma and loss of consciousness, but will dissimulate as regards the convulsions. She asks for remedies applicable to the symptoms which follow the attack, but will not allow us to suspect the sad truth. I have often been consulted by persons who knew perfectly well they were subject to epilepsy, but who spoke of nothing but congestion; wives conceal the state of their husbands, and husbands that of their wives; and still more frequently parents dissimulate with regard to the symptoms presented by their children. The medical man is, then, constantly deceived with regard to epilepsy. He is deceived by the patient himself who knows nothing of his attack, except that he had lost consciousness, and that he remained for some hours in a state of semi-stupidity. He is deceived by his relations, who own with great reluctance, even to themselves, that they have an epileptic among them. He is deceived by the remembrance of his first medical education, during which he has been repeatedly told that apoplecticiform cerebral congestion is a common disease. Do not let us be astonished, therefore, that congestion is so generally believed in.

There is, I admit, a form of convulsion which may be mistaken for a cerebral congestion. It happens, though very seldom, that at the commencement of an epileptic attack, the tonic period—that is to say, that during which the muscles of the chest preserve an absolute rigidity; it happens, I say, that this period may last two or three minutes, instead of lasting only fifteen to thirty seconds, and the individual dies of asphyxia, just as death in tetanus takes place during a spasm; or as animals die when poisoned by strychnine as our colleague M. Ségalas demonstrated so well nearly forty years ago. As there are in this case no clonic convulsions with which those who are ignorant of our art are most familiar; as, during the whole course of the tonic convulsion the face has been turgid, and the vessels of the neck distended as if in knots; as, in fact, there is great congestion, but a congestion passive, analogous to that which exertion produces; it is believed that we have to deal with an active congestion, whereas it is nothing else but an attack of eclampsia or epilepsy. If our colleagues who have to deal with the diseases of pregnant women and children appeal to their memory, they will probably coincide in my opinion.

There is yet another disease to which the name of congestion is constantly

applied, viz., vertigo in connection with gastric derangements. This strange form of nervous disease is characterized by the following phenomena:—If the patient make a sudden movement in bed, he feels the bed turning, and, as it were, dragging him in its movements; if he rise, and above all, if he look up, the giddiness becomes more serious; objects turn round, he staggers, and is sometimes unable to stand, and at the same time experiences a severe pain in the precordial region, as also in many cases, vomiting. These singular symptoms are, by the patient, called *coups de sang*, and, further, most medical men share that opinion. They bleed, apply cupping-glasses and leeches, order foot-baths with mustard; and, in fact, do all that they can to cause the disappearance of the imaginary congestion, which they increase by their strange treatment.

The forms of vertigo of which I have spoken are akin to syncope, and, consequently, are exactly the opposite of congestion; and, however absurd it may seem, it is nevertheless true that too many medical men still mistake a tendency to syncope and confound it with cerebral congestion. However, as I wish to exaggerate nothing, I will suppose that the two states which I have indicated are rarely mistaken by medical men; and I will suppose further that they are never taken for cerebral congestions. But there is an accident which frequently accompanies hæmorrhage of the brain, and which by most medical men is considered as a congestion. Thus, when a person is struck with apoplexy, whether the apoplexy has its cause in cerebral hæmorrhage, or depends on softening, or results from embolia, or at least from a sudden obliteration of one of the principal arteries at the base of the brain—when, I say, a person is struck with apoplexy, there is sometimes a sudden loss of consciousness, and the impairment of the intellect and of motion lasts some hours or some days, and then there is a complete recovery or a slight hemiplegia, which gradually diminishes, and finally disappears in a few months. As the first effects have been almost like a thunderbolt, and as there does not appear to be a sufficient relation between the gravity of these first phenomena and the consequent derangement of intellect, sensation, and motion, it is said that the cerebral hæmorrhage has been accompanied by congestion, and that the congestion, a phenomenon essentially transitory, has produced, properly speaking, *apoplectic* effects; and that these being dissipated, there remains of the serious apoplectic results nothing but a trifling hæmorrhage with slight hemiplegia.

I do not wish absolutely to deny the existence of this congestion, and I avow that I am inclined even to admit it to a certain extent; but there is another phenomenon which, in so far as I know, has not attracted sufficient attention; I mean that which I have named *étonnement cérébral*. When the encephalon suddenly undergoes a rupture and compression, it supports that grave lesion with an impatience which varies according to the individual, but which may, in some cases, go to a great extent. For example; if a soldier receive a bullet in his head, or if in a scuffle an individual receive a stab which penetrates to the brain, they are felled to the ground as if struck with a club; but gradually, in spite of the intracranial flow of blood which is the result of the wound, and even in spite of the inflammatory congestion inseparable from that rupture of the tissues, intelligence, sensation, and motion are often restored with a strange rapidity, thus affording to the inexperienced surgeon hopes which, unhappily, are not realized. That that immediate stupor, or that which I have called *étonnement cérébral*, however incorrect that term may be, always exists, is a fact which can be contested by no one. Experiments on animals give still more positive results. If we trephine the cranium of a dog or of a rabbit, and by an incision in the *dura mater* introduce a small bullet between the cranium and the surface of the brain, we observe, first, the phenomena of stupor, which are rapidly dissipated, to be replaced by a hemiplegia proportioned to the degree of compression. In this experiment we cannot induce cerebral commotion, and we

must therefore believe that the encephalon is in some manner surprised by an accident which declares itself by these transitory results. Am I not then right in supposing that, when a sudden escape of blood takes place into the *corpus striatum* or the *thalamus opticus*, the immediate stupor which is generally attributed to simultaneous congestion, may, in part at least, be imputed to this *étonnement cérébral*?

It must not be supposed that I absolutely disbelieve in cerebral congestion. I admit that congestive hyperæmia of the brain may exist; and further, that it would be absurd to contest the fact of its existence; but what I say is this, that what has been called *apoplectiform cerebral congestion*, is, in the majority of cases, an epileptic or eclamptic attack, and sometimes syncope. I say also that very often attacks of simple epileptic vertigo, or of vertigo connected with gastric derangement or ear disease, are erroneously viewed as congestion of the encephalon.

If the propositions which I have endeavoured to defend be true, it will be conceded that the revulsive and antiphlogistic medications incessantly called into play to combat imaginary cerebral congestions should be less frequently used, and that other indications should be looked for, more conformable to the idea that we should search for information with regard to different conditions, which have been too often confounded under the same denomination.—*Gazette des Hôpitaux*.

II.—MANNER OF PRACTISING SYPHILIZATION IN THE HOSPITALS OF CHRISTIANIA, WITH THE RESULTS OF THAT MODE OF TREATMENT. BY PROFESSOR W. BOECK.

M. BOECK takes the virus of a chancre contracted by coitus, or of a pustule or artificial ulcer from those who are under treatment by syphilization. He makes the first inoculations in the sides, three on each side; and in three days three other inoculations on each side, using this time the matter from the pustules produced by the first inoculation. Every three days he continues to inoculate in the sides as before, always using the matter of the last pustules, until he arrives at a result absolutely negative. He then commences to inoculate the two arms, and continues in the same manner as with the sides, until the matter produces no further effect in this new situation. On reaching this point he takes the matter from another individual, and inoculates the sides, or the arms, or both these places at once, continuing with this new matter as with the first, until it has no further action. When there is immunity for the second matter both in the sides and arms, he commences inoculation of the thighs, and continues in the same manner as in the former situations, until the effect ceases. On the three places indicated, he makes fresh inoculations with new matter, and proceeds as before until no matter whatever has any further effect.

Several recent authors having stated that the hard chancre does not produce inoculable matter, M. Boeck states as the result of his experiments—

1st. That the soft, as well as the hard chancre, may yield a matter which is not inoculable.

2nd. That the soft chancre yields inoculable matter much more frequently than the hard chancre.

3rd. That the matter of the soft chancre is, in general, much more intense than the matter of the indurated chancre.

M. Boeck states that the inoculation ought to be continued as long as it is possible to obtain results with any syphilitic matter whatever, and that the

inoculations should be made with great exactness at the fixed times. If the inoculations be discontinued before complete immunity has taken place (even if the syphilitic symptoms have disappeared), or if it be done irregularly, relapses are apt to occur.

He has observed, also, that when infants affected with hereditary syphilis are inoculated, the first inoculations do not take in most instances. In this case they are repeated every day until they commence to take; when it generally happens that not only the last, but also the whole series of previous inoculations, produce simultaneously pustules which, as well as the succeeding ulcers, stand, as regards their inoculability, in relation to the age and the development of the child. It often, however, happens that at a later period the inoculations do not follow the same regular sequence as in acquired syphilis; the positive result often ceases after a somewhat short series of inoculations, and may be recommenced after two or three weeks. The consequence is, that the treatment takes in general more time. In order to calm the children, one centigramme ($\frac{1}{15}$ of a grain) of the watery extract of opium is given three times a day. During the treatment in hospital, M. Boeck does not alter the ordinary diet, and in his private practice he prescribes a generous diet, interdicting only the use of spirits.

M. Boeck has established it as a principle, that syphilization should only be practised in the case of those who are affected with constitutional syphilis. "If mercury," says he, "has been employed before, syphilization does not, in general, proceed with regularity; the treatment is longer, and there is a greater tendency to relapse. The same is the case after the iodide of potassium. Syphilization acts with more promptitude and certainty, the sooner that it is employed after the appearance of constitutional symptoms."

The following are his statistics:—Of 204 individuals treated by syphilization, there died 16 children with hereditary syphilis; 1 child with acquired syphilis, and 1 woman aged fifty. Of these 16 children with hereditary syphilis, 14 died shortly after the treatment was commenced; 1 in two days, 1 after nine days, and the others from 14 to 35 days afterwards. Only 2 were treated for a longer period; the one, treated for 62 days, died of lobular pneumonia, and the other, treated for 166 days, succumbed to an attack of phlegmonous erysipelas of the scalp. The child attacked with acquired syphilis was treated for 29 days, and died of croup, after undergoing the operation. Of the 182 individuals cured by means of syphilization, 19 returned to hospital; of these, 7 were again subjected to syphilization and were cured; 1 had a second relapse; 3 employed, while the syphilization was going on, the iodide of potassium; and 2 were treated in town for relapse by the iodide of potassium: 12 of the 19 who returned were not syphilized, but were treated by means of mild remedies, several of them for a very short time. The average time of treatment for the 182, including the relapses, was 144 days; 1 had typhoid fever during the syphilization, 1 pneumonia, and 1 measles; maladies which greatly aggravated the syphilis. Several of the women syphilized bore syphilitic children, but some had healthy children. All of them enjoyed, without exception, after the treatment, at least as good health as before it.

To the 204 syphilizations practised in the circumstances which we have indicated, must be added 90 cases of a like nature treated by Drs. Baumann of Lillehammer; Danielssen and Bull of Bergen; Gjor, Holst, and Tiedmand of Christiania; Heiberg of Drobak; Wildhagen of Drammen, &c., which make the total basis of these statistics 294.—*Gazette des Hôpitaux.*

III.—ON THE TREATMENT OF ECZEMA. BY PROFESSOR HEBRA.

ONE has the satisfaction, Professor Hebra observes in his Clinical Lecture, of knowing that we can always cure eczema, however long it may have continued, although in some inveterate cases depending upon a dyscrasia, or upon internal causes, there may be considerable difficulties in the way. As in most other diseases of the skin, the treatment should be purely local, internal measures being limited to the rare cases in which the eczema has been produced by a previously diseased condition of the economy, or when it is combined with some other affection. In the great bulk of cases internal means, such as mercury, antimony, iodine, purgatives, sarsaparilla, &c., are superfluous and mischievous. Arsenic is the only one of such means which exerts any influence in obstinate cases. Ordinarily, however, it is of no use, and its employment should be limited to the few cases which manifest especial obstinacy, the local treatment in these also not being at the same time neglected.

Cold water, in its various modes of application, is of great importance, combined with other means, in the treatment of eczema. Employed alone it is far too tedious, and frequently not sufficing in its operation. It only aggravates the malady when applied in *eczema simplex*, arising from excess in secretion, as e.g. in the axilla, between the buttocks, &c. Starch, whether alone, or mixed with oxide of zinc (starch 3j, zinc ʒiij) is, on the other hand, an excellent application when the eczema arises from the friction of two cutaneous surfaces, or from excessive secretion, as in the axilla, under the breasts, the scrotum, buttocks, &c. Oxide of zinc (3j, to ʒj of lard), sulphate or acetate of zinc, alum (3j to ʒij of water), red or white precipitate (6 to 12 grains to ʒij of lard), are of good service in acute eczema or in chronic when there is but slight infiltration and the disease prevails only over a limited extent. When, however, there is considerable infiltration in chronic eczema these means do not suffice, and then the almost indispensable *schmierseife*,* by reason of its slightly caustic action, becomes the most preferable remedy. Its mode of employment varies according to the degree and extent of the eczema. When there is but slight infiltration, a rubbing with it once or twice a day is sufficient; but when the infiltration is more considerable, a more frequent application is necessary, and even epithems composed of it may be required. When we have thoroughly rubbed the skin with the soap, until excoriations and red points have been produced, the surface should be washed and cold applications laid on until next rubbing. This procedure must be continued as long as the moisture and itching and the infiltration of the skin continue, and until the frictions no longer give rise to heat and excoriations of the skin. The soap must then be replaced by cold applications, and the treatment terminated by the employment of tar.

In many chronic cases, with great infiltration, the soap does not suffice, and we must have recourse to a stronger form of potash, viz., caustic potash, 1 drachm dissolved in 2 drachms of water. A pencil of charpie is dipped in this and well rubbed into the diseased parts for some minutes. These are then washed, and cold applications are kept constantly employed. One or two such cauterizations usually suffice, and when more are necessary they should not be repeated oftener than once a week. Carefully applied, the caustic gives rise to no cicatrix, but the practitioner must himself always undertake its application. It is a very painful procedure, and is not often required. Cauterizing with strong acids is to be avoided, as it causes great pain and gives rise to scars. The application of the nitrate of silver is of no avail. Tar is in its way just as useful a remedy as the soap, the time for its application being when the moisture

* For an account of this "*schmierseife*," or *sapo viridis*, vide *Medical Times and Gazette*, 1860, vol. i. p. 224.

and itching have ceased, and exfoliation has commenced, *i.e.* when *eczema squamosum* is present. It may be used either alone or mixed with equal parts of cod-liver oil, and should be applied by means of a brush once or twice a day, carefully avoiding washing the parts or allowing water to come into contact with them. As long as any redness or desquamation continues, the tar must be repeated. Sometimes, when the application of the tar has been premature, moisture and itching are observed at certain spots, and the preliminary treatment has then to be resorted to again. Some individuals cannot bear the tar at all, it giving rise to severe inflammatory action. An ointment of acetate of lead or oxide of zinc should in such cases be substituted. *Cod-liver oil* is a valuable external application, and by its aid alone we are able to cure the eczema when this has not lasted very long and the infiltration is not very considerable. It is also an excellent adjuvatory to the treatment by *schmierseife* and cold applications, as flannels soaked in the oil may be kept bound over the diseased parts during the night. Employed alone, the treatment is very tedious, and is objectionable on account of the disagreeable smell and befouling the linen which it gives rise to. Taken internally it does not exert the slightest influence on eczema.—*Wiener Spitalzeitung*.

IV.—NEW METHOD FOR OBTAINING A NEUTRAL AND INALTERABLE SOLUTION OF THE PERCHLORIDE OF IRON.

IN spite of the numerous processes published of late years, we are far from being possessed of a solution really neutral and inalterable; and hence grave accidents are from time to time happening during the administration of this precious agent, which call to mind this *desideratum*. An able pharmaceutical chemist of Paris, M. Adrian, in a work addressed to the Académie, has taken up anew the study of this question. The following is the process, as sure as it is elegant, which M. Adrian recommends to his colleagues in order to obtain the official solution of the perchloride of iron:—

The process which I, have for long adopted for the solution of the neutral perchloride of iron is very simple. It consists in preparing by means of hydrochloric acid and small nails (*pointes de Paris*), a solution of protochloride of iron of 25° Baumé. In order to avoid any oxydation of the iron, the solution is at once poured into a series of Wolf's bottles, through which a current of well washed chlorine is passed for about five or six hours. This time is usually sufficient to convert all the protochloride in the first vessels into perchloride. The last bottles of the series, which are not saturated, are now put in the place of the first, and the latter are filled with a fresh portion of the solution of protochloride, if we wish to render the process continuous. The solution of perchloride of iron poured into a porcelain capsule, is now submitted to a temperature which ought never to exceed 50° for an hour. Towards the end of the operation a current of air is passed through the fluid, in order to remove the last traces of chlorine which may remain in solution. The liquor obtained is about 25° to 30° Baumé, and it is brought to the desired degree of concentration either by prolonging the evaporation, or by adding distilled water.

The perchloride of iron obtained by this process is chemically neutral, which is proved by the following analyses; and solutions kept for a long time have not undergone the slightest alteration; whereas the perchloride obtained by other processes becomes altered within a few days of its preparation.

The normal and chemically neutral solution of perchloride of iron is perfectly clear without ever having been filtered; it deposits, even through time, no

ochre-like sediment, and its colour is a saffron-yellow. It has great astringency, without the acid taste of all the other solutions.

It contains at 30° Baumé—

Water,	74
Anhydrous perchloride of iron,	26

Mean of three analyses of the normal solution of neutral perchloride of iron at 30° Baumé.

Per centage found by operation on 2 grammes of solution.

		By calculation.
	Chlorine,	16·989 16·959
Oxide of iron,	12·80	Iron,
		8·960 8·976

These results are all in favour of this process, and prove that the perchloride of iron obtained by following the precautions which I have indicated is quite chemically neutral.—*Bulletin Général de Thérapeutique*.

V.—TREATMENT OF INFLAMMATORY AFFECTIONS OF THE FEMALE BREAST. BY W. H. BYFORD, M.D., OF CHICAGO.

AFTER fully describing the character of these affections, Professor Byford comes to their treatment. He arranges that for inflammation of the nipples under the heads of prophylactic, palliative, and curative. The nipple must be prepared for its duties. The causes operating upon it produce abrasions, and their action is facilitated by the natural and acquired tenderness of the structures, particularly the epidermis and skin. Hence these must be hardened. The nipple should be covered lightly during pregnancy and nursing; the thinner and more permeable the covering the better. It should freely admit the air. At the same time the organ "*should be subjected pretty constantly to moderately rough friction*."

An excellent dressing for the nipples for the last two months, is a rough, coarse sponge, so cut as to cover the areola; surround and cover loosely, but touch every part of the nipple. Over this there should be but one thickness of clothing, so as to allow of the evaporation of fluid as fast as secreted, and the free admission of air. In cold weather, of course, the parts should be covered more when going out. The nipples should be occasionally moistened with water, and allowed to dry slowly; friction with a dry towel or the fingers will assist.

During lactation the same rules should be observed, and after nursing, the nipple should be wiped clean and dry before being covered. A little glycerin or olive oil will prevent cracking. When inflammation comes on, palliatives and curative measures are demanded. The healing process being continually interrupted by the performance of the functions of the organ, it is necessary to protect the part from the effect of these interruptions.

Artificial means are required, which intervene between the mouth of the child and the nipple. For this purpose the shield must be employed. This should be made in the form of a conical hat, having a rim, a crown cavity, with a draught tube rising out of the top for the passage of the milk. This rim should be large enough to cover the areola, the crown passing over the nipple, merely touching it on the sides. If the abrasions are on the summit of the nipple, the shield should be so deep that, when drawn, the top of the organ will not touch, or else

it will cause pain. But if the cracks are on the side or base of the organ, then the cavity of the shield must be shallow, so that the top of the nipple touches its bottom in such a manner as to prevent any stretching, and to bring the pressure entirely on the top. In this latter case, the bottom of the cavity should be as smooth as possible, and correspond in shape to the summit of the nipple, in order to prevent unequal pressure. A soft linen rag, properly adjusted over the draught tube, is preferable to any other envelope.

M. Legroux mentions the following ingenious contrivance. He applies this mixture:—

R.—Collodion, p. xxx;
Ol. Ricini, p. ss;
Ol. Terebinth. p. jss.

which is quite adhesive, and dries less quickly than collodion, on the areola with a brush, so as to encircle, but not touch, the nipple for the width of an inch. While yet soft the nipple is covered with gold-beater's skin, which is pressed well down upon the mixture. Thus is formed a smooth and pliant covering. Holes are pricked through the skin with a needle, to allow of the passage of the milk. Before sucking, this must be moistened with sugar and milk.

The curative means for sore nipples are various. The same will not do for abrasions as well as ulcerations. Nature is to be imitated by forming a cuticle for the part.

Abrasions may be covered with starch and mucilage. The following is a good mixture:—

R.—Cerat. Alb. oz. ij;
Ol. Amyg. Dulc. dr. j;
Mel. Despum. oz. ss. M.

Dissolve with gentle heat, and add Bals. Canad. dr. ijss.

Apply each time of nursing. When the cracks are deep, close them by pressing their edges together, and covering with collodion in a thick and wide coat; this must be renewed when found necessary. When ulceration exists, it will be acute or chronic. Act as for this affection elsewhere; deplete, if acute, by leeches, and apply cold emollient poultices; or envelop the nipple in a thin layer of thick mucilage, covered by oil-silk, so as neatly to fit the organ, kept cold by ice applied in a bladder. When these remedies are not necessary, apply mucilaginous and bland ointment applications. Alum and tannin are good at first; sulphate of zinc and borax come next in respect to time. One scruple of tannin to one ounce of rose-water, five grains of alum, or sulphate of zinc are useful in the early stages, when the acute symptoms are subdued.

The following are useful:—

R.—Sodæ Subborat. dr. ss;
Glycerin, dr. ij;
Aq. Rosar. f. oz. jss. M.
Use as a wash after sucking.

R.—Sodæ Subborat. dr. ij;
Cretæ Præp. oz. j;
Spt. Vini,
Aq. Rosar. ãã f. oz. iij.
Mix and dissolve.

The latter may be used when the ulcer is becoming indolent. In the chronic form strong astringents and stimulants become necessary. A skilful use of the sulphate of copper and nitrate of silver will shorten the course of these ulcers.

The latter applied solid to the surface, not oftener than once in eight days, is excellent. In the interval the sore may be dressed with tannin or alum in solution. When irritable, an ointment may be used, made of belladonna, hyoscyamus, or opium. One very good expedient, which will often entirely change the character of the ulcer, is to anæsthetize the part with ice, as practised prior to operating.

When the lymphatic glands become affected, antiphlogistic measures must be employed; and when chronic, alteratives, tonics, liniments, &c., according to the peculiarities of the case. The treatment of milk abscess is of great importance. It should be prevented, if possible, by proper management at the outset. When the nipple is deficient, or, from any cause prehension is deemed impossible, it is decidedly improper to attempt nursing. In other cases, prolonged and judicious efforts should be made to render the organ useful. The first thing is to take perpendicular pressure off the top of the nipple, by some device to prevent the dress from forcing it in, and this, if possible, should be commenced early in pregnancy. For this purpose a shield should be employed, which will cause a pitting of the anterior surface of the breast, and a projection of the nipple. When called upon to treat a rudimentary nipple, after parturition, the effect must be more prompt. In many cases the organ may be made available by causing it to erect itself by simple titillation by the finger, and immediately applying the child; or by placing a thick layer of collodion around it on the areola, which, drying, elevates the nipple. Then, by keeping the reservoirs empty, abscess is prevented. To aid in this we have various tubes and pumps, but all of which are objectionable. A puppy is often used, but it likewise is liable to irritate and excoriate the nipple. The only proper way is by the mouth of an adult, varying the pressure or force to suit the tenderness of the part.

A very useful class of measures are those to suppress the secretion, and thus relieve the distention, as opium in large doses, or applied as an ointment; but belladonna seems to have acquired most renown. Numerous instances are reported of its great value in such cases. Much depends upon its strength and application by inunction till the production of its characteristic effects upon the system. Cold, as a local remedy, is beneficial. The temperature of the breasts for this purpose should be kept steadily at about 40 or 45 degrees, as by water running through an india-rubber bag enveloping the organ, or the application of a bladder. No bad effects are to be apprehended from it. Internally, a saline cathartic may be given every other day, and two grains of iodide of potassium every four hours will materially assist.

Acute inflammation, the effect of congestion, is apt to be extensive, and will require energetic treatment. Warm fomentations may be applied for the first few hours with the hope of establishing the secretion of milk. A decided venesection will often turn the balance in favour of resolution. Immediately after this, the use of the *veratrum viride* may be commenced in doses of six drops every four hours, till the pulse is brought down below the normal standard, and then kept there. One grain of calomel, with a quarter of a grain of sulphate of morphia, may be given, if the pain is urgent, say every four or six hours. A lotion of one part of sulphuric ether to two parts of alcohol will be a good soothing adjunct, after the inflammation becomes permanent. These measures should not be abandoned for warm poultices until suppuration is clearly evident, by which plan we may often limit the extent of this process. In this state of the gland, the most moderate means only should be employed to draw the breast. Retained milk is not the cause of inflammation here, as in milk abscess. If glandular inflammation is complicated with that of the reservoirs, the treatment for both must be combined, as local and general antiphlogistics with means to arrest the secretion and empty the reservoirs. Chronic inflammation will be cured by treatment similar to that for other glandular inflammations, as leeches,

mercurials, iodine, and vegetable alteratives, internally and externally. Much reliance can be placed upon well-regulated and graduated pressure with adhesive straps, pressing the diseased part against the ribs; or with collodion thoroughly encasing the breast. When pus forms, evacuate it early, though where the abscess is deep, it is desirable to wait until the pressure from within has caused condensation of the overlying tissues, otherwise a large opening will be required. In milk abscess the earlier and smaller the opening the better. The effect of suppuration and evacuation of a milk reservoir is often to destroy its cavity, but in some cases a milk fistula is formed. This may be closed by an occasional application of the nitrate of silver. Worse than these are the tortuous lacunæ, that sometimes result from the deep glandular abscess of the breast, and which are generally very difficult to cure. Injection of iodine is most to be relied upon. This may be done by inserting a soft, flexible catheter to the bottom of the canal and throwing the injection through it so as to apply it without dilution to the bottom of the fistula. This favours the shallowing instead of the narrowing of the cavity. Of course it is never advisable to slit up these obstinate puriferous ducts, because of the amount of tissue that might be damaged, which it is desirable to save.—*Chicago Med. Exam.* Sept. 1860.

VI.—A CASE OF GUN-SHOT WOUND; BULLET FOUND IN THE WALL OF RIGHT VENTRICLE OF THE HEART EIGHTEEN YEARS AFTER THE ACCIDENT. By G. B. BALCH, M.D.

A SHORT history of the case is as follows:—In June, 1842, an Irish boy, by the name of John Kelly, received an accidental shot in his right shoulder; the ball passed through three inch-boards before it struck him. A surgeon was called, who probed the wound, and found the ball lodged nearly under the inner third of the clavicle. The ball entered the shoulder through the upper border of the trapezius muscle, about an inch and a half or two inches from the acromion process. There was not much hæmorrhage at the time, and the surgeon did not deem it prudent to remove the bullet, and in about six weeks the boy was able to be at work. This accident occurred at Chatham Four Corners, Columbia Co., N.Y. In 1844 Mr. Kelly came to this county (Clinton), where he has since resided.

Fourteen years ago he was taken very dangerously ill with pneumonia, accompanied with a very severe and irregular palpitation of the heart. Dr. Terry, who attended him at that time, says he did not expect his recovery. Ever since that sickness his heart has shown symptoms of organic disease, at times beating in such a tempestuous manner, that one standing ten or fifteen feet from him could see its action very distinctly. Ever since he was shot he has had strabismus, and at times inflammation of the right eye.

His last sickness was caused by his going into the water, ten days before his death, and taking a severe cold; his heart then commenced its actions with redoubled fury, accompanied with dysphonia, and severe pain in his shoulders and arms. His right arm became purple and cold before death.

On Friday, 15th June, 1860, I made the post-mortem examination, by request of Dr. Terry, who was his attending physician. The autopsy revealed a condition of things I did not expect. The right subclavian artery was filled with ossific matter at the thyroid axis; the other arteries were healthy. The right internal jugular and subclavian veins were enlarged; the right external jugular was closed near its union with the internal; I found the remains of the vessel where it entered the internal jugular.

The upper lobe of the right lung was congested. There were no tubercles in the lungs, but there was considerable pleuritic adhesion.

The heart was enlarged, and undergoing fatty degeneration. The pericardium was very adherent; so much so that I could not separate it from the heart, without cutting either one or the other. At the lower part of the right ventricle I felt a hard lump. I passed my finger into the right ventricle, and found the lump to be in the wall of the ventricle, near its lower part. I then cut with my scalpel from the outside down upon the lump, and found it to be a leaden bullet, slightly flattened.

Now the query arises, How long had this bullet been in the heart? I will not advance any theory of mine; I think the facts of the case tell the story, without any theorizing.—*American Medical Monthly*, September, 1860.

VII.—NEW OPERATION FOR THE RADICAL CURE OF INGUINAL HERNIA. By D. HAYES AGNEW, M.D., Surgeon to the Philadelphia Hospital.

Two cases of inguinal hernia of long standing have been cured by Dr. Agnew's method, one occurring in his own practice, the other in that of Dr. Garretson. The instruments necessary for the operation consist, first, of two semi-cylinders of steel, three inches long and one inch and a quarter in circumference, which can be separated from each other by a screw in the handle of the instrument, and on the internal face of the lower blade of which are two parallel longitudinal grooves. Secondly, of a spear-pointed needle, slightly curved at the extremity, and supported on a bone handle. Third, of several stout needles from two to two and a half inches in length.

A portion of the scrotum is carried into the external abdominal ring, followed by the metal cylinder, and thus thrust up to the internal ring. The blades are now separated by means of the screw attached to its handles, and the long needle, armed with a thread of silver wire, is carried along one of the grooves to the upper end of the cylinder, and then made to pierce the exterior parietes of the inguinal canal; the thread is then removed from the needle, the latter withdrawn, and the other end of the wire passed through its eye, when it is passed along the other groove and made to emerge upon the surface of the skin, a short distance from the first. It is again unthreaded, and after the removal of the needle the two ends of the wire are drawn up tightly and twisted over a small roll of lint. This effectually holds up the plug of integument to the very summit of the canal, and as the silver thread manifests but little disposition to ulcerate out, it may be allowed to remain for a considerable time. The second and most important step of the operation consists in screwing the handles of the instrument completely together, thus separating the blades in the canal to their greatest possible extent, and then carrying across the canal, between the blades, four or five threads at equal distances from each other. The first thread should be composed of silk, and be introduced as near the internal ring as possible; the remainder should be of silver, and the last one be close to the external ring. These transverse threads can be lodged in the canal with great accuracy by this method, the cord being protected from injury by the posterior blade of the instrument. The latter being withdrawn the patient is confined to his bed, the parts being protected by a compress and roller, and the wires should be removed so soon as the plastic exudation has bound the invaginated plug to the walls of the canal, which in the cases operated upon required twelve days.

The patient of Dr. Agnew was fifty-five years of age, who had an oblique inguinal hernia of seventeen years' standing; that of Dr. Garretson occurring in a lad fourteen years of age, who had been affected with an oblique inguinal

hernia, which had descended into the scrotum, since his second year. In both cases the parts remain perfectly invaginated and indurated, and the patients are walking about. The compress is still worn to favour firm consolidation.—*Medical and Surgical Reporter*, Nov. 17, and Dec. 1, 1860.

VIII.—EXPERIMENTAL RESEARCHES RELATIVE TO A SUPPOSED NEW SPECIES OF UPAS. By WILLIAM A. HAMMOND, M.D., Professor of Anatomy and Physiology in the University of Maryland.

Among the most deadly poisons which the ingenuity of man has devised for purposes of destruction, must be placed the two well-known species of upas employed by the natives of the Indian Archipelago. Whilst woorara, corroval, and vao are used by the aborigines of a portion of the western continent, as death-dealing agents, the Javan possesses poisonous compounds, scarcely, if at all, inferior in virulence to those mentioned, and around which, for a long period, hung as much mystery as was ever attached to their American analogues.

* * * * *

The more obvious effects of the poison are shown in the following experiments:—

Exp. Ten drops of the diluted poison were inserted under the skin of a large cat. During the first two minutes no symptom worthy of note was observed. Then slight twitchings of the muscles of the back were perceived, and immediately afterwards the animal vomited. At the end of four and a half minutes the cat fell, and was tetanically convulsed. Death ensued immediately, in the midst of violent and general tetanic spasms. Circumstances prevented me making any further examinations in this case, and it is only adduced as being among the first of the series of experiments, and as showing with what great power the poison acted.

Taking, however, into consideration the results of the chemical examination, and the extreme rapidity with which death followed the first convulsions in the experiment just cited, I was strongly of the opinion that some other cause than tetanus was active in producing the ultimate result. I therefore proceeded as follows:—

Exp. Three drops of the poison were inserted under the skin of the back of a large frog, and the chest immediately laid open, in order to observe the action of the heart. At the time this organ pulsated fifty times per minute, and with great regularity. After the fourth minute the movements became more irregular, the ventricle contracted to less than half its normal size, lost its deep colour, and finally ceased to beat, five minutes after the inoculation. The auricles stopped beating a few seconds subsequently. The frog, however, was still possessed of a good deal of muscular vigour, and was able to leap several feet. The animal was placed under a bell-glass, and carefully observed. Seven minutes after the movements of the heart were arrested there were slight convulsive actions of the abdominal muscles, and in less than a minute afterwards the frog was in violent tetanic spasms. These were excited by the least irritation. They continued for nearly half an hour with undiminished violence, and then gradually ceased. Fifty-five minutes after the inoculation the animal was dead to all excitation.

Exp. Ten drops of the poison were introduced under the skin of a medium-sized cat. At the end of seven and a half minutes the animal had a slight spasm and vomited. In two minutes afterwards it fell, with general tetanic convulsions, and expired. The chest was immediately opened, and the heart was found to have ceased acting.

These experiments were frequently repeated, and always with analogous

results. They lead inevitably to the conclusion that the poison used, besides inducing tetanus, acts directly upon the heart. In fact, in frogs its first effect is to arrest the action of this organ, and it is not till some minutes have subsequently elapsed that the tetanus supervenes. It is probable that the same is the case in mammals, but, owing to the rapidity with which the poison acts in warm-blooded animals, it is difficult to arrive at a very definite conclusion on this point. The following experiment, which was several times repeated with like results, shows, at any rate, that the heart is affected before any spasms occur:—

Exp. Fifteen drops of the poison were inserted into the thigh of a small dog. The chest was immediately opened, artificial respiration being kept up by means of the apparatus described in the memoir on *corroval* and *vao*. The action of the heart was not more than ordinarily disturbed till about four minutes had elapsed. Portions of the left ventricle then became paralyzed, and the pulsations of the organ became much slower. Up to this period no convulsions had occurred; but shortly after the appearance of the phenomena above referred to a slight tetanic spasm of the whole body took place. In less than a minute subsequently—about six minutes after the inoculation—the heart suddenly stopped, simultaneously general tetanic convulsions occurred, and the animal was dead.

It may therefore be concluded that the poison referred to in this memoir, like the upas antiar, corroval, vao, and tanghin, acts primarily upon the heart, but, unlike these agents, acts also upon the spinal cord, causing tetanic convulsions. Its action would therefore appear to be a compound of that of the two known species of upas, and it might be supposed that the poison in my possession was a simple mixture of these substances. In order to discuss this point properly, it will be necessary to return to the chemical examination made.

It will be recollected that 20 grains of the poison yielded 4.75 grains of an exceedingly bitter substance, possessing, in some respects, the characteristics of strychnia, but yet differing very materially from it in several essential particulars, and that this matter was further separated into strychnia and a substance soluble in water.

The solution of this latter ingredient, on being carefully evaporated to dryness in the water-bath, left a light yellow substance, extremely hygroscopic, and possessing a somewhat astringent and slightly bitter taste, the latter being probably due to traces of strychnia. It was slightly soluble in alcohol, and very much so in ether, differing therefore, in these respects, from antiarin. It was also readily dissolved by chloroform. It was altogether uncrystallizable, another point of difference from antiarin.

In physiological properties it appeared to resemble this latter substance. A small portion, not larger than the head of a pin, arrested the action of the heart of a large frog in about four minutes. A pigeon, inoculated under the wing, fell dead in two minutes. On opening the chest, the heart was found hard and rigid. In neither case was there the least appearance of tetanic or other convulsions.

The quantity of this substance becoming exhausted, I was unable to experiment further with it. The chemical properties were certainly not those of antiarin, and therefore the idea that the poison to which this memoir relates is a mixture of the upas antiar and the upas tieute is not tenable.

We have already seen that the primary action of the poison under consideration is upon the heart, and that secondarily it acts upon the spinal cord, producing tetanus. That this last mentioned result is not a consequence of the arrest of the heart's action will be admitted by all who are familiar with the results which follow ligature or removal of this organ in frogs. In such cases there are no convulsions, the animal dying from complete abolition of all nervous action. From numerous experiments which I have performed with reference to

this point, I have satisfied myself, that after placing a ligature around the large vessels at the base of the heart, on extirpating the organ, the voluntary movements entirely cease in from twenty to fifty minutes, and the reflex in from one to two hours. My experiments in this respect are therefore entirely in accordance with those of Kölliker already referred to.

* * * * *

In its effects upon nervous and muscular irritability, the poison under consideration is more closely allied to strychnia than to corroval or antiar, i.e., the action of the strychnia is predominant over that of the other principle, and consequently the nerves and muscles retain their excitability for a longer period than when either of the two above-named substances is introduced into the circulation. Without giving the details of the experiments, I would state as the results of my investigations, that in frogs the nerves retain their irritability for about five hours, and the muscles for about an hour longer.

When taken into the stomach, the two principal actions of the poison are reversed in the order of occurrence. Tetanus first occurs, and it is not for some time afterwards that the heart stops beating.

Exp. Ten drops of the solution of the poison were introduced through a tube into the stomach of a large frog at 10.20. Tetanic spasms commenced at 10.32, and at 10.40 were at their height. The chest was opened, and the heart was found to be actively pulsating. It continued to beat till 10.58, when it stopped.

Exp. Twenty-five drops of the solution were injected through a tube into the stomach of a small dog. At 12.5 the chest was opened, and artificial respiration instituted. Tetanus supervened at 12.13. The heart continued acting till 12.28, when it stopped.

I found, by subsequent investigation, that it was possible to entirely prevent the paralysis of the heart by washing out the stomach a few minutes after the introduction of the poison. This is shown by the following experiment:—

Exp. Ten drops of the solution of the poison were placed in the stomach of a frog at 2.15. At 2.20, before tetanic spasms had become developed, the stomach of the animal was inverted, and thoroughly washed with tepid water. It was then returned to its normal position. During the operation tetanus supervened. The chest of the animal was opened, and the heart was found pulsating actively. It continued beating for several hours, during the whole of which time the convulsions were excited on the least irritation.

Introduced into the rectum, the effects ensue in the same sequence as when the poison is placed in the stomach, but with somewhat greater rapidity. It may, therefore, be concluded that the mucous membrane of the alimentary canal is a better endosmometer for the solution of strychnia, than for that of the heart-paralyzing agent.

Placed upon the skin of frogs, the poison produces similar effects to those which follow its insertion under the skin, and with almost as much rapidity. It is perhaps hardly necessary to cite any of the numerous experiments which were performed with reference to this point.

It was intended to have made these investigations much more extensive, but the limited quantity of the poison at my disposal prevented me doing as complete justice to the subject as I desired. From the researches, so far as they extend, I think it may be fairly concluded that the poisonous substance to which they relate is altogether different from any one species of poison heretofore described, and that whilst in many respects it is similar in physiological effects to both the upas antiar and the upas tieute in their joint actions, there is much reason for hesitating to regard it as a compound of these substances.—*The American Journal of the Medical Sciences*, October, 1860.

MEDICAL INTELLIGENCE.

I.—*Annual Report of the Glasgow Royal Infirmary for 1860.*—In accordance with the terms of the charter and the usual custom, the Managers of the Glasgow Royal Infirmary have now to submit to this general court of contributors and subscribers the following report of their transactions for the past year, which forms the Sixty-sixth Annual Report of the Infirmary:—

In the *first* place, they will give an account of the number of patients treated in the various departments of the house, with the numerical results as to recovery and death.

In the *second* place, they will relate the more important matters which have come under their consideration during the past year; and—

In the *third* place, they will give an account of the money which they have received on behalf of the institution, and the amount and manner of its expenditure.

The number of patients remaining in the medical and surgical wards on the 31st December, 1858, was 258. The number admitted throughout the year was 3149; so that the total number treated was 3407. Of these, 3139 were treated to a conclusion, of whom 2884 were dismissed cured, relieved, or from other causes, and 255 died. The number remaining on the 31st December, 1860, was 268.

In the fever wards the number of patients remaining on the 31st December, 1859, was 54. The number admitted during the year was 588; so that the total number treated was 642. Of these 603 were treated to a conclusion, of whom 516 were dismissed cured, and 87 died. The number remaining on the 31st December, 1860, was 39.

In the whole institution the number of patients remaining on the 31st December, 1859, was 312. The total number of patients admitted throughout the year was 3737; so that the total number treated was 4049. Of these, 3742 were treated to a conclusion, of whom 3400 were dismissed cured, relieved, or from other causes, and 342 died. The number remaining on the 31st December, 1860, was 307.

The smallest number of patients on a single day in the medical and surgical wards was 190, on the 5th August; the largest number was 279, on the 19th January.

The daily average number, as calculated for each month, was least in July, being 207; and greatest in February, being 264.

In the fever wards the smallest number of patients on a single day was 17, on the 15th August; and the largest was 86, on the 11th April.

The daily average number, as calculated for each month of the year, was least in August, being 22; and greatest in April, being 72.

In the whole institution the smallest number of patients on a single day was 214, on the 20th of August; the greatest number was 345, on the 9th of March. The daily average number, as calculated for each month in the year, was least in July, when it was 229; and greatest in February, when it was 328.

The number of out patients treated in the dispensary was 10,811, of whom 5133 were surgical, and 5678 medical cases. In this department the number of patients has been nearly 1000 in excess of last year. The recent change by the increase of medical officers has contributed to give to it greater efficiency and usefulness.

The number of children vaccinated during the year was 987, which is 248 more than last year. The total number vaccinated during the three past years

has been 2809, being an average of nearly 1000 annually. A large supply of vaccine lymph was secured, and freely distributed to medical men both in Glasgow and the country.

Small-pox prevailed with a degree of unusual severity in the first half of the year, which induced the managers, in conjunction with other bodies, to bring the matter again under the notice of the Lord Advocate, with the view of urging upon him the propriety of bringing into parliament a measure to render vaccination compulsory. Their efforts, however, in this direction have hitherto failed to produce a successful result.

The following table affords a

**COMPARATIVE VIEW OF THE MOVEMENT IN THE INFIRMARY DURING
THE YEARS 1855, 1856, 1857, 1858, 1859, 1860.**

MEDICAL AND SURGICAL WARDS.

	1855.	1856.	1857.	1858.	1859.	1860.
Remaining at the end of preceding year,....	191	245	270	235	211	285
Subsequently admitted,	2604	2659	2880	2712	2776	3149
Total treated,.....	2795	2904	3150	2947	2987	3407
Cured, Relieved, &c.,.....	2318	2396	2664	2508	2498	2884
Died,.....	232	238	251	233	231	255
Remaining at the end of year,	245	270	235	211	258	268
Smallest number on any day,.....	162	183	185	186	181	190
Largest do. do.	268	278	303	287	278	279
Greatest daily average for a month,.....	256	268	283	267	257	264
Smallest do. do.	182	195	202	202	188	207

FEVER WARDS.

	1855.	1856.	1857.	1858.	1859.	1860.
Remaining at the end of preceding year,....	94	88	60	43	37	54
Subsequently admitted,	860	787	787	510	535	588
Total treated,.....	954	875	854	553	572	642
Cured and dismissed,	738	702	696	448	446	516
Died,.....	128	106	115	68	72	87
Remaining at the end of year,.....	88	67	43	37	54	39
Smallest number on any day,.....	34	84	23	12	9	17
Largest do. do.	108	103	112	59	59	86
Greatest daily average for a month,.....	95	94	103	54	47	72
Smallest do. do.	41	38	29	18	14	22

Though the number of cases admitted into the fever wards during the past year exceeds that of the two preceding years, when it touched the lowest point reached during the last thirty-five years, the increase is so inconsiderable—the number ranging between 500 and 600 cases—that we may consider this city, during the three years by-past, with a population more than trebled, in a better condition in relation to fever than it was in 1817–18–19, when the average for three years was over 900 cases annually, or during any three years from 1825 to 1854, when the annual average ranged from 1000 to upwards of 5000.

The unusually large number of patients admitted into the fever wards affected with small-pox, a number greater than on any former year in the history of the infirmary, makes the number of ordinary fever cases appear larger than last year; but if we deduct these and a considerable number of miscellaneous cases of disease admitted into the fever wards, the amount of fever will be found to have been unprecedentedly small.

The admissions to the medical and surgical wards during the past year have exceeded by several hundreds the number admitted during any previous year. They amounted to 3149. The next highest number was in 1857, when it reached 2880. The great severity of the weather contributed to keep the wards more fully occupied during the summer months than is usual at that period of

the year. The number of cases of accident admitted has been upwards of 600, and is higher than on any former occasion.

The number of surgical operations has also been unusually large—many of them have been of the most serious description, and they have generally been attended with very considerable success. The extent of the injuries received has, however, on many occasions, precluded any hope of benefit from surgical interference.

The sanitary condition of the wards has been excellent. The measures adopted in previous years have been carried out with the best effect, in causing the absence of those hospital diseases which are the result of over-crowded and ill-ventilated wards.

The attention of the managers has been chiefly occupied during the past year with the erection of the new Surgical Hospital. Commenced in 1859, the work was much retarded during the early months of last year by the inclemency of the weather, but was prosecuted with vigour during the summer and autumn. It is now approaching completion, and could, if occasion required, be speedily put into a state fit for occupation. Though the contracts originally entered into amounted only to £6770, it was not expected that that sum would suffice for the completion of an undertaking of so great a magnitude. In consequence of modification and changes in the original contracts, such as plastering the walls with cement instead of common plaster, and new contracts for additional work, such as building boundary walls, making a new entrance and lodge in Castle Street, laying out the grounds, &c., considerable additional expense has been incurred, and it is probable that the whole work will not be completed for a sum less than £9000. When compared with the expense incurred in previous additions to the infirmary buildings, the new hospital will be found to have been constructed at a very moderate cost, more especially when the very superior and spacious nature of the accommodation provided is considered. The cost per bed, allowing over 1000 cubic feet for each patient, will not exceed £50.

The ground floor, which affords a large amount of excellent accommodation, and no part of which is intended to be occupied by patients, is not included in this estimate of the accommodation. It will be employed for nurses' dormitories, store-rooms, and other necessary purposes.

The following are the principal features of the new building:—It is built in a straight line, with a slight projection behind, for clerks' and nurses' rooms. It has a central staircase, with the wards, eight in number, branching off to the right and left. The wards occupy the whole breadth of the building, and have seven windows on each side opposite to each other, and reaching from near the ceiling to four feet from the floor. The space occupied by the windows is one-third of the area of the side walls. The beds will be arranged along the walls, and will have nearly one window for each bed. The same general arrangements prevail throughout the older portions of the infirmary. In the new building, however, the wards are more spacious in all their dimensions, are better lighted, and possess more efficient means of ventilation. They are 14 feet high, 60 feet long, and 28 broad within the walls; being higher by 2 feet, longer by 10 feet, and broader by $4\frac{1}{2}$ feet than the wards in the fever and front house. There are 24,304 cubic feet of space in each ward, which would give more than 1000 feet for 24, and upwards of 1500 for 16 patients. The walls are plastered with Portland cement and finished with Keane's, which gives a hard, polished, and non-absorbent surface.

The heating is effected by open fires placed in the centre of the ward, at each end of an oblong shaft, and in the most favourable position for radiating heat to all parts of the ward. This central shaft is of sufficient dimensions to contain all the smoke vents, and a large space in its centre (4 feet by $3\frac{1}{2}$) is subdivided into separate ventilating flues, for the removal of the foul air from the wards. There is a large opening into it on each side near the ceiling, which gives direct

egress to the contaminated air. Besides these there are two openings into the smoke flues near the ceiling, which, together with the two open fire-places, form six apertures for the removal of the air both from the highest and lowest levels.

Fresh air is admitted by 35 apertures in each ward, not including the doors. There are six openings in the ceiling, communicating with corresponding openings in the walls, three on each side, by channels between the joists; these open at regular intervals on either side of the beam which runs along the centre of the ward. When the upper half of each window is pulled down 2 inches, the air is directed towards the ceiling by means of a channel lined with wood. In like manner when the lower half of the window is thrown up 2 inches, the air does not enter directly into the ward, but passes down behind the lining of the window recess, and enters on a level with the floor. By this means the angles formed by the walls with the ceiling and the floor—the parts where the air is usually stagnant—can be readily flushed with fresh air. There is a large opening above each ward door, provided with louvres which can be opened or shut at pleasure. By these means, both for the extraction of foul air and the admission of fresh air, an unusual amount of ventilating power has been provided, which may be used wholly or partially as occasion may require. It may be observed that both the heating and ventilation of each ward is separate from, and independent of that of all the other wards.

The managers have also provided a supply of hot water by boilers placed in the basement at each end of the building.

A separate ventilation is established for the sculleries, bath-rooms, and water-closets.

A large and commodious hoist has been erected, by means of which patients may be conveyed to the different floors.

A new operating theatre has been constructed on the upper floor of the building, with accommodation for upwards of 200 students.

The only other feature of the new building which may be mentioned here, is the provision of day rooms or parlours for the use of patients who are not confined to bed.

The removal of the wooden building erected in 1847, which was effected in the course of the year, has largely added to the extent of the green, around which the various buildings of the infirmary are placed. It now extends to 1½ acres, and the relative position of the buildings is such as to permit of the freest ventilation in every direction. The ground behind the new building, an acre in extent, and that in front of the infirmary, partly inclosed and partly open to the public, with the Cathedral grounds to the east, and Castle Street on the west, secure freedom of external ventilation, and provide against undue encroachment of buildings in every direction, and for all time coming.

The new entrance in Castle Street will be found to give more convenient access than previously existed to all parts of the infirmary.

The completion of the new hospital will enable the managers, besides providing accommodation for the increasing number of patients, to allot to each bed such an amount both of cubic space and surface area as to secure the best sanitary condition of the wards, and to reserve the fever hospital exclusively for that class of cases for which it was originally intended.

In order to simplify the rules as to the election of the medical officers in the house, on the occasion of vacancies, and to assimilate to these the rules as to the election of medical officers in the dispensary, the following resolution was adopted by the managers in regard to the medical officers both in the house and dispensary:—

In the event of vacancies occurring during the year, the same shall be temporarily supplied by the election of a physician or surgeon duly qualified, who shall hold office until the first Monday of November thereafter, and such physi-

cian or surgeon shall be eligible for future election; and at the next ordinary meeting for election purposes, the managers shall elect a physician or surgeon, duly qualified, for the period specified in, and in terms of, the by-law enacted with regard to the medical attendants in the house, at the annual meeting of contributors and subscribers in 1858, provided always, that if such vacancy shall occur after the ordinary meeting for election purposes, the managers may elect a physician or surgeon, qualified as aforesaid, both temporarily, and also in terms of the foresaid by-law.

The managers have now to lay before this court an account of the financial affairs of the infirmary during the past year. The details of income and expenditure will be found in the financial statement and table of household expenditure appended to this report.

Of the income, the first and chief item consists of the annual subscriptions, which have this year amounted to £3479 8s., which exceeds the sum received last year by £111 7s., and that received the year before by £62. The managers, while gratefully acknowledging this increase on the most important part of their annual income, confidently anticipate, with the prospect of increased expenditure, when the new hospital is put into operation, that their efforts to extend the accommodation and provide for the comfort of the sick and the injured, will be supported by increased liberality on the part of the community.

The managers have much pleasure in being able to report a considerable increase in the amount of contributions from the operatives employed in the various manufacturing establishments of the city, and in the mining districts of the neighbourhood. The sum received from this source has been £2251 7s. 9d., which is an increase of £207 over last year, and only £1258 less than the sum received from annual subscriptions.

The amount contributed by the captains and crews of the Clyde steamers is £113 16s., and is an increase of £4 over last year.

The total amount received from public works and steamers has been £2365 3s. 9d.

This large sum contributed by the working classes—for whose benefit when sick and injured the infirmary is maintained—is a source of encouragement and gratification to the managers, affording as it does the best testimony of their appreciation of the value of the institution.

The next item of the income of the infirmary is that derived from legacies, which has this year amounted to £1902 1s. 6d. Of this sum £1125 is a farther payment to account of the legacy left by the late James Ewing, Esq. of Strathleven. The following are the principal legacies received during the past year: £250 left by the late Miss Arthur of St. George's Road; £100 by the late James Richardson, Esq. of Ralston; £100 by the late James Anderson, jun., Esq. of Port-Glasgow; £100, part of the residue of the estate of the late Captain Wishart, Glasgow; £55 left by the late William Provan, Esq., wood merchant, Glasgow; £50 by the late Alex. Graham, Esq. of Lancefield; £50 by the late Daniel Gilchrist, Esq., Maryhill, besides various smaller sums. From the legacy account of last year there falls to be deducted the sum of £360, which was repaid this year to the executors of the late Graham Hutchison, Esq., it having been found that by an oversight his legacy had been overpaid to that amount.

In addition to the above-mentioned sums received from legacies, the managers have gratefully to acknowledge £311 15s. 10d. received as donations, of which the principal items are £100 from "A Friend abroad," per Daniel Forbes, Esq., 45 West George Street; and £40, the balance, after paying expenses, of the Tricentenary meeting held in the City Hall on the 20th December.

The sum derived from students' fees has amounted this year to £1443 15s., which far exceeds the amount received from this source on any previous year. The number of students attending the infirmary has been gradually increasing during the last few years, till it is now more than double—a fact which speaks

for itself as to the estimation in which the infirmary is held as a means of medical education. The great increase of the present year is undoubtedly owing, to some extent, to the contemplated changes in the curriculum of medical education. One-half of the sum received from students' fees is divided amongst the clinical lecturers.

Under the head of expenditure the principal sum consists of house expenses, which have this year amounted to £8604 2s. 10d. This is £1000 more than was expended last year, and is accounted for chiefly by the increased number of patients.

The sum paid to this date on account of the new building has been £7877 19s. 5d., and it is anticipated that £1200 further will be required to finish the building.

In conclusion, the managers have to offer their thanks to the various officers of the institution. To the physicians, Drs. Tannahill, Bell, Ritchie, Orr, Fraser, and Cowan; and to the Surgeons, Drs. Eben Watson, A. Buchanan, Lyon, Morton, Geo. Buchanan, Dewar, and M'Leod, for their valuable services. To the treasurer, Mr. John Jamieson, for his zealous and efficient services in the management of the financial affairs of the infirmary. To Mr. Lamond, *interim* secretary, and to Mr. Wylie, cashier and collector, for the faithful and diligent discharge of the duties of their respective offices.

To their excellent superintendent, Dr. M'Ghie, the managers have to tender their best thanks, not only for his general management, but for the many valuable suggestions received from him in the progress of the new building, and in his attention in bringing under their notice every useful improvement in hospital accommodation. Their thanks are also due to Mr. Morton, chaplain, and to Mrs. Struthers, matron.

The managers have also to convey their thanks to the City Corporation for the gratuitous supply of water, and to those coal proprietors who have sent in donations of coals.

Mr. Lamond, *interim* secretary, read the financial statement annexed to this report.

Mr. Dalglish, M.P., moved that the financial statement and report now read be approved of, and the by-law recited in the report be confirmed, and the thanks of this general court tendered to the managers for their valuable services; which motion, being seconded by Thomas Buchanan, Esq., was unanimously agreed to.

The Deacon Convener moved that the special thanks of this court be given to the operatives of the city and neighbourhood, and seamen, for their continued subscriptions in aid of the funds of the charity; which motion was seconded by W. W. Watson, Esq., and unanimously agreed to.

The *interim* secretary then declared the managers *ex officio* for the ensuing year to be as follows, viz. :—

The LORD PROVOST.

The MEMBERS OF PARLIAMENT FOR THE CITY.

The DEAN OF GUILD.

The DEACON CONVENER.

Dr. JOHN M'FARLANE, Professor of Medicine.

Dr. ALLEN THOMSON, Professor of Anatomy.

Dr. WILLIAM LYON, President of the Faculty of Physicians and Surgeons.

And he reported the following appointments :—

BAILIE GEMMEL, by the Town Council.

JOHN M'EWEN, Esq., by the Merchants' House.

JOHN MORRISON, Esq., by the Trades' House.

Dr. J. M. PAGAN, by the College.
 Rev. Dr. BOYD, by the Ministers of Glasgow.
 Dr. COATS,
 Dr. A. D. ANDERSON, } by the Faculty of Physicians and Surgeons.
 Dr. JAMES WATSON, }

And the qualified contributors present elected the following ten directors in terms of the charter, the first eight being unanimously re-elected on the motion of the chairman, and the two last in room of those retiring by rotation, in conformity with the rule established by the general court in 1843, being elected on signed lists, by a majority, viz. :—

JAMES CRAIG, Esq.	JOHN JAMIESON, Esq.
BAILIE WHYTE.	ROBERT SMITH, Esq.
JOSEPH A. WRIGHT, Esq.	JOHN WILSON, Esq.
ALEXANDER RONALDSON, Esq.	DAVID SMITH, Esq.
WILLIAM KERE, Esq.	WILLIAM EUING, Esq.

2. *The Twenty-Sixth Annual Report of the Glasgow Lying-in Hospital and Dispensary.*—The Directors, in submitting the Twenty-sixth Annual Report of the Glasgow Lying-in Hospital and Dispensary to the subscribers and to the public, have the satisfaction of stating that the charity continues to answer in every respect the ends for which it was instituted, as the following details will show :—

The number of women confined in the hospital from 15th November, 1859, till 15th November, 1860, was.....	811
The number of women confined in their own houses during the same period was..	324
Total,.....	635

Maternal deaths—In hospital,	4
“ “ Out of hospital,	6

Children born in hospital alive—Boys, 154; Girls, 140,	294
“ “ “ still-born at full time—Boys, 8; Girls, 6,	14
“ “ “ premature—Boys, 6; Girls, 4,	10
Total number,.....	318
(There having been seven twin births.)	

Children born at their mothers' houses alive—Boys, 165; Girls, 134,	299
“ “ “ still-born, at full period—Boys, 11; Girls, 6,	17
“ “ “ premature—Boys, 6; Girls, 7,	13
Total number,.....	329
(There having been five twin births.)	

CAUSES OF MATERNAL DEATHS.

Of the four women who died in hospital, one died from convulsions, one from exhaustion after excessive hemorrhage with placental presentation, one from puerperal phlebitis, and one from long-standing thoracic disease. Of the six women who died out of hospital, three died from puerperal peritonitis, two from exhaustion consequent on difficult and protracted labour, and one from cause not ascertained.

Præternatural and operative cases in hospital,	23
" " " out of hospital,	15
Total number,	38
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Number of Scotch women confined in hospital,	171
Number of Irish women confined in hospital,	138
Number of English women confined in hospital,	2
Total,	311

The number of patients who have applied for admission to the hospital, and also of those who have sought out-door attendance, has been somewhat fewer than for several previous years. This diminution is chiefly to be accounted for by the removal of the hospital from the district of the town inhabited by the poorest and most wretched part of the population, who freely availed themselves of its benefits when they could be procured at their very door, but whose habits are such that they will not put themselves to the trouble to seek these benefits at a short distance. This is distinctly seen in the fact of the patients having been of a better and more deserving class than formerly.

The new hospital, corner of Rottenrow and Portland Street, which, as mentioned in last report, was then nearly completed, was opened for the reception of patients towards the end of January, and has been found to be well calculated for the purposes of the institution. The situation is salubrious; the rooms are of sizes well suited for obstetric patients, and the various necessary appliances, both internally and in the outer premises, have been found to answer well. The supply of hot water, in particular, to various parts of the house has been a great comfort and advantage, affording the means of personal cleanliness, in many cases where otherwise it would have been neglected.

Of the four deaths in hospital only one arose from a form of disease at all likely to spread among lying-in women. Of course this patient was immediately, on the appearance of her malady, separated from the others (for which we have now every facility); and it is gratifying to think that, although childbed fever has for some time been more than usually prevalent in the city, we have only had one case. The patients, an unusual proportion of whom, as will be seen from the above statement, had severe and complicated labour, generally recovered quickly and completely.

The expense of supporting the institution will now be considerably more than formerly; but this is amply compensated for by the greatly-increased advantages and comforts which it affords. Through the liberal donation of friends the debt which was incurred in the purchase, necessary alterations, and additions to the building, has been considerably reduced. The directors beg to assure these donors of their warmest thanks. A sum, however, is still wanting to paint the hospital, and to supply many articles of furniture and bedding requisite for obtaining the full advantage of our additional accommodation. The directors anticipate they will at no distant period be able to realize the necessary funds for accomplishing these desirable objects.

The claims of the institution have never been obtruded on the public; but the directors feel confident that the benevolent and the discerning have only to be made aware of the misery that is mitigated, the aid and comfort that is afforded to so many in an hour of trial and peril, and it may be the crime that is prevented through the means of the Lying-in Hospital, to bestow on it their cordial countenance and pecuniary support.

The directors have to express their regret at the loss the institution has sustained by Mr. W. C. Alston, one of their number, having gone abroad. He had for many years taken an active part in the management, and in everything

connected with the establishing of the new hospital his services had been very valuable.

The directors beg to offer their sincere thanks to the subscribers to the institution. They were much gratified to receive a second donation from the trustees of the late John Ferguson, Esq., of Cairnbrock, and also a handsome subscription from the Roman Catholics of the city, per the Right Reverend Bishop Murdoch.

To those ladies who have sent articles of cast-off clothing they are much indebted. The matron finds such things at all times truly useful, but the supply of them is often quite inadequate. She therefore begs the ladies of Glasgow for donations of babies' clothing, for which they may have no further use, and of articles which are required in child-bed.

The directors have to thank James Merry, Esq., M.P., and Messrs. M'Naughton & Hood, for donations of coals, and David Smith, Esq., for a donation of shoes.

The directors also vote their thanks to the Medical Officers, Treasurer, and Secretary.

3. *Fifty-fifth Annual Report of the Glasgow Lock Hospital for 1860.*—At the general annual meeting of the qualified contributors and yearly subscribers to the Glasgow Lock Hospital, held within the Religious Institution Rooms, on Wednesday, 16th January, 1861—John Couper, Esq., in the chair—the following report was read:—

In presenting the fifty-fifth annual report, the directors have to express their satisfaction at the continued prosperity of the institution. The receipts during the past year have exceeded those of previous years. Considerably more interest has been excited generally in the hospital, and the opportunities it presents for assisting and benefiting the inmates; while attention has been turned to it in a sanitary and reformatory point of view—its effect undoubtedly being to prevent the spread of disease, and to diminish crime and pauperism. The directors acknowledge with peculiar pleasure the assistance granted by the magistrates and police authorities in awarding £21 17s. 6d. out of the fines levied in the police-courts. The contributions from annual subscribers have slightly increased; but, however gratifying this may be, there is still need for sustained exertion and liberality. In a large city like this, former subscriptions naturally cease, from deaths or removals to the country; the directors are compelled, therefore, constantly to ask for new and additional subscriptions. The hospital is entirely supported by voluntary contributions. It is not generally so popular or so well known as it deserves. It has, however, strong claims upon public support. Many of the unfortunate creatures who are treated are without friends or home—exposed to much temptation—without time to reflect on their condition; and, while in the hospital, an opportunity is given them, by Christian instruction and judicious counsel, to begin reformation. It is a gratifying feature, in this year's transactions, that 71 patients have been taken care of after leaving. Of that number, 45 were taken in charge by one benevolent lady, who desires her name to be concealed, but whose kindness and humanity cannot be too strongly commended; 12 cases have been admitted into the Magdalene Asylum, and 14 have been restored to their friends. By the extension of these redeeming means, much good may be looked for. The directors cannot refrain from referring to the continued labours of Mr. Michael Rowan in regularly visiting the patients, and attending to their spiritual welfare. By a reference to the statement appended, there is still a balance due upon an heritable bond on the property of £800, which the directors consider it desirable should be paid off at as early a period as possible.

The following is the Report of the Medical Officers.—During the year just closed there were 21 more patients treated to a conclusion than during the previous

year, and that, it will be observed, at the small additional gross outlay of £6 5s. In consequence of the severe and protracted character of their ailments, several patients made long sojourn in the hospital, which has to a small extent raised the average period of the residence of each, the increase being from 29½ nights to 31 and a fraction; yet it is pleasing to observe that the average expense to the institution of each patient treated, is diminished since last year. There have been three deaths in the house; two of these were infants. The third case was admitted by mistake, the ailment under which the patient laboured not being of the description treated in the Lock Hospital, and she was too ill to admit of her being removed. The medical officers have again to express their perfect satisfaction with the mode in which the superintendent and his wife have performed their responsible duties, and also their appreciation of the valuable nurse whose services are secured by the hospital.

On the motion of the chairman, the report was adopted; and the thanks of the meeting unanimously awarded to the medical officers, treasurer, and secretary, for their services during the past year.

On the motion of Mr. John Mitchell, seconded by Mr. M'Kenzie Kirkland, the following list of directors for the year 1861 was agreed to:—

Rev. Dr. M'Taggart,	-	-	-	From Clergy.
Messrs. John Mitchell,	-	-	-	" Town Council.
A. M'K. Kirkland,	-	-	-	" Merchants' House.
David Yuile,	-	-	-	" Trades' House.
Drs. James Fraser and J. G. Wilson,	-	-	-	" Faculty of Physicians and Surgeons.

Messrs. George Anderson, Walter Gray, J. F. Jamieson, Matthew Pearston Bell, John Couper, James Lumsden—From qualified contributors and yearly subscribers.

The following were re-appointed:—Dr. George H. B. M'Leod and Dr. Robert Perry, acting surgeons. Robert Finlay, Esq., honorary secretary, Dr. John Aitken, treasurer, Mr. Robert Young, secretary—directors *ex officio*.

4. *Annual Report of the Glasgow Eye Infirmary.*—The directors of the Glasgow Eye Infirmary have continued satisfaction in laying before the contributors the Thirty-seventh Annual Report of the institution. During the past year the business of the infirmary has increased in extent, and has fully maintained its efficiency and success; 1649 new cases have been admitted, which, with 618 remaining on the roll at 31st December, 1859, make a total number of 2267 cases treated, being a considerable excess over the number of the previous year. For the medical and financial details the directors refer to the detailed statements annexed.

During the past year some improvements have been made in the internal arrangements of the infirmary, and further alterations and additions to the building of the infirmary are contemplated, with the view of extending the accommodation for the large and increasing numbers who take advantage of the institution. Of course additional expense will thus be entailed, and the directors trust to the liberality of the public to second their endeavours to keep the infirmary abreast of the increasing requirements made upon it. Past liberality encourages the directors to rely with confidence that means for any improvement will not be withheld.

A special appeal to the working classes was last year drawn up and circulated among the public works. The directors are thankful to say it has been, to some extent, successful, additions having since been made both to the number of works contributing and to the amount contributed. The sum received from public works last year was £163 9s. 8d.; that for the preceding year was £135 11s. 1d. While gratefully acknowledging this liberality, the directors cannot help thinking that the contributions from this source should be still

greater. Though the appeal circulated was described as a "special" one, it was no temporary emergency which dictated it. The call for aid is unceasing, and the directors trust that in future years they will still be able to record a progressive increase of liberality.

Patients treated during 1860.

Remaining on the list, 31st December, 1859,	618
Admitted since,	1649
	<hr/>
	2267
Dismissed cured,	1407
" " by operation,	86
" relieved,	73
" " by operation,	4
" incurable,	12
" " after operation,	2
" irregular,	81
" " after operation,	2
" with advice,	4
Death,	1
	<hr/>
	1672
Remaining,	595

5. *Hospital for Sick Children.*—A meeting to consider the propriety of establishing an hospital in Glasgow for sick children was held on the 23d of January, Robert Dalglish, Esq., M.P., in the chair. The following were among the reasons which were urged in favour of the establishment of such an institution:—

Of the 33,674 deaths registered in Glasgow in the three years ending 1859, 17,897, or considerably more than one half, were of children under five years of age. The serious question came to be, What did the children die of? and, putting aside hereditary causes, it was found that eight diseases produced one half of the mortality—namely, hooping-cough, scarlet fever, bronchitis, pneumonia, diarrhoea, enteritis, and small-pox. Four of these were admittedly contagious, and if children affected by any of these diseases could be removed from a crowded dwelling to an hospital, much might be done to diminish the mortality. The first thing that would be secured by removing them to an hospital, was effective medical treatment; and it was well known that many of the children of the poor never had medical attendance when ill, except when too late to be of any avail. In the next place, the removal of such children to an hospital would be valuable as a preventive of disease; and, further, such an hospital would afford an excellent school for the clinical study of the diseases of children, and for the training of nurses. There were hospitals of this kind in all the large towns of the continent, and also in London, Manchester, Dublin, Liverpool, and Edinburgh.

Several of the gentlemen present expressed an opinion strongly in favour of the proposed scheme; and before the meeting separated, a committee was appointed to take, without delay, the necessary steps towards the establishment of an hospital.

We are glad to say that a large sum has already been subscribed, and that we are now almost in a position to consider the establishment of an hospital for children in Glasgow as a *fait accompli*. We hope ere long to be able to announce to our readers, that an hospital worthy of this great city has been thrown open for the sick and suffering children of our poor.

6. *Dispensary for Skin Diseases.*—We are glad to be able to inform our readers that a dispensary for the treatment of skin diseases has been lately established in Glasgow. This has been done for the purpose of devoting more particular

attention to skin diseases than has hitherto been bestowed upon them here, for of all affections, those which manifest themselves on the external covering of the body have been most neglected." In London there are three or four flourishing institutions for their treatment, and one of these afforded advice and alleviation to no less than 10,000 patients in the course of one year. On the continent, where the highest success in the treatment of these maladies has been attained, the facilities given for relief and study have doubtless produced this result—as in Vienna, where a number of beds are set apart in one of the principal hospitals for skin disease, and a professor appointed by government for the purpose of teaching their diagnosis and treatment. In Paris, also, there are no less than 400 beds at the St. Louis hospital for such diseases, and the number applying for relief at the dispensary averages from 100 to 200 daily.

The subscriptions to the Glasgow dispensary for the first year amount to about £180, which is quite sufficient for carrying it on in the modest manner in which it was deemed prudent to commence a novel undertaking; but we trust that, by the end of a year, its proved utility may entitle it to such enlarged support as to enable it to extend its operations by fitting up baths, in order to give them in desirable abundance to patients. This can only be done at present on a very small scale. The dispensary (63 Upper John Street) is open Monday, Tuesday, Thursday, and Friday, at 2 o'clock, P.M. At present there is no admission to students, as the institution is not yet in working order; but we understand that it is at all times open to medical men.

7. *Quarterly Report of the State of Disease in the Glasgow Royal Infirmary for the Quarter ending 25th March, 1861.*—In the medical and surgical wards the number of patients remaining on the 1st January was 268; the number admitted since has been 857, so that the total number treated has been 1125. Of these 766 have been dismissed cured or relieved, and 59 have died; the number remaining on the 25th March was 300. The rate of mortality has only been 1 in 19, or about 5 per cent. of the total treated.

In the fever wards the number of patients remaining on the 1st January was 39; the number admitted since to the 20th March has been 169, so that the total number treated has been 208. Of these 125 have been dismissed cured, 25 died, and 58 remained on the 20th March. The rate of mortality has been 1 in 8, or about 12 per cent.

The number of accidents has been 120.

BOOKS RECEIVED.

The Ætiology, Pathology, and Treatment of the Congenital Malformation of the Rectum and Anus. By William Bodenhamer, M.D. New York: Samuel S. and William Wood, 1860.

Clinical and Pathological notes on Pericarditis. By W. T. Gairdner, M.D., F.R.C.P. Edinburgh, &c. Edinburgh: Sutherland and Knox, 1861.

On Insufficiency of the Aortic Valves in connection with sudden death. By John Cockle, M.D., F.L.S., &c. London: J. W. Davies, 1861.

On the Waxy or Amyloid Degeneration of the Kidney. By T. Grainger Stewart, M.D., formerly resident Physician Royal Infirmary, and President of the Medical Society of Edinburgh. Edinburgh: Murray and Gibb, 1861.

On Some of the Causes of the high rate of Mortality in Greenock. By James Wallace, A.M., M.D.: Greenock, 1860.

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ORIGINAL COMMUNICATIONS.

I.—*Observations on Placenta Prævia.* By CHARLES CLAY, M. D., Manchester; late Senior Medical Officer to St. Mary's Hospital Manchester; Lecturer on Midwifery and Diseases of Women and Children, &c.

AMONGST the many forms of uterine hæmorrhage presented to the notice of the obstetrician, that arising from placental attachments more or less to the os uteri is one especially calling forth the mental energies of the practitioner, and to the successful treatment of which his coolest judgment and skilful manipulation will be necessary. In the earliest ages of medicine, the danger of this peculiar kind of hæmorrhage was fully appreciated. Hippocrates himself wrote pointedly on its fatality and importance, and subsequent ages, with all the experience brought to bear upon it, have failed to lessen its serious character, though it is hoped the amount or percentage of its previous high rate of mortality has been, by improved modes of treatment, very considerably lessened. It is almost unnecessary to refer to the opinions and practice of the early ages of medical writers on this question, except to state that Guillemeau advocated immediate delivery; Mauriceau, and La Motte entertained very peculiar and erroneous notions as to the cause or precise nature of the hæmorrhage in these cases, supposing that the placenta had previously occupied a higher position within the uterus, and had subsequently fallen from thence to its present position—an absurdity we shall not here stay to discuss, as it could not add any thing new to the practical treatment of such cases. Portal describes this form of cases, and advises speedy delivery, Gifford, Smellie, Røederer, and others, advise the same means to be adopted. Levret insists on immediate version.

In 1822 Kinder Wood, Esq., of the Manchester Lying-in Hospital, with whom I commenced my own professional career, had observed cases in which the placenta was attached wholly or partially to the os uteri, and in which, when the placenta chanced spontaneously to separate, immediate cessation of the hæmorrhage followed, both child and placenta being subsequently expelled by the simple efforts of nature unaided by the accoucheur, and without any bad consequences, the patients generally doing very well, and making reasonably rapid recoveries. From these facts he inferred, that by simply detaching the adhered placenta from the os uteri by the forefinger, the case might safely be left to nature for completion in almost all cases; and that the result would be far less dangerous than the usually accepted plan of version, which is not always applicable without a great amount of violence, and even in its most desirable view is attended by a large amount of fatality, even when unaccompanied with the extensive hæmorrhage that usually characterizes cases of placenta prævia. One of Mr. Wood's strongest points of argument rests on the *now well ascertained fact* that in all cases, immediately after the detachment is effected, the hæmorrhage *as certainly ceases*. Another point equally forcible is that detachment can be accomplished at much earlier stages of the dilatation of the os uteri—when only the finger can be admitted—and before any great prostration can have taken place, and this at a time when it would be the height of imprudence (which is the mildest possible term that could be applied to such a proceeding) to forcibly dilate an unwilling and almost undilatable os to the extent of introducing the hand into the uterus with the view of version. The amount of violence done to both mother and child is excessive, a point abundantly proved and confirmed over and over again by the high rate of mortality displayed in the statistical returns of various authors. Indeed the percentage of mortality shown on version *in all cases is so very high*, that, without the necessary increase applied to those cases where extensive hæmorrhages accompany it, it cannot be justifiable to increase the mortality if any possible means can be suggested for its reduction, and which I trust will be clearly shown in these remarks.

Mr. Wood's plan of detachment was not only proposed, but put into practice for a considerable period before 1822, and with invariable success; an example that was not lost on his pupils, myself amongst the number. I have, for nearly forty years, continued the same practice with almost entire success, except in one or two solitary instances, where the distance travelled was great, and consequently the loss of blood, and time, had produced an unconquerable amount of prostration. In 1845

Professor Simpson of Edinburgh, whose indefatigable exertions for the improvement of the profession are beyond all praise, again opened the general question, and a lengthy controversy arose between him and Dr. Radford of Manchester as to priority of views. Mr. Wood's previous claim was almost lost sight of during this controversy; and this was the more remarkable as Dr. Radford had been the colleague of Kinder Wood for many years, and was perfectly well acquainted with his mode of practice.

If we turn to the statistical records derived from a considerable number of authors, we find that cases of hæmorrhage from placenta prævia are about 1 in 480 to 500 cases. This does not include cases where hæmorrhages existed, and where a spontaneous separation of the placental mass had caused the hæmorrhage to cease altogether, *but such cases only* where manual interference was called for, proving its adhesion to the os wholly or partially. The statistics of version and immediate delivery in cases of placenta prævia are about as follows:—

Fatal to mother,.....	1 in 3 Cases.
Fatal to child,.....	1 in 2 “

These returns appear at first sight excessive, but from the best information I have been able to obtain, I am of opinion that the points are too favourably stated. I have conversed with many practitioners of extensive experience, who generally considered the fatality to mother and child, each, to be not less than 50 per cent., and which I believe to be nearer the truth.

Now from the best authorities who have written in defence of detachment and then leaving the case to the efforts of nature—Professor Simpson, Dr. Radford, myself, and others—I find it,

Fatal to mother,.....	1 in 44 Cases.
Fatal to child,.....	1 in 5 “

Compare for a moment these two tables, and who can hesitate where to place the confidence? Then, in support of this practice there is another striking fact. In all cases of detachment, the success according to the reports of Professor Simpson, &c., is thus stated in reference to the hæmorrhage:—

Failures,.....	1 in 20 Cases.
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In other words, in 19 cases out of 20 the hæmorrhage immediately ceases the moment detachment is effected. In my own practice I have never known it to fail; I have only known a case or two that have not recovered, but it has been from the prostration before the detachment, and not from any continuance of the hæmorrhage.

That the returns above given are not miscalculated, I appeal to the statistics of version and immediate delivery from all causes. I have now before me the returns of many practitioners and public institutions, amounting to 173,847 cases of midwifery, of which 1555 were version cases. From all causes we find—

The fatality to mother was,.....1 in 14½ Cases.
 " to child,.....1 in 3 "

Then again, as placenta prævia occurs but once in 480 to 500 cases, and version from all causes once in 112 cases, it follows that four fifths of cases of turning are independent of placenta prævia, and even these cases are accompanied by a large amount of fatality, but nothing in comparison to the mortality of version in cases of placenta prævia when taken by themselves.

If we view this question in another position we find it still worse. Suppose instead of waiting a reasonable time until a sufficient amount of dilatation be effected, and the os uteri is in some measure disposed to dilate, which according to many authors is said to be about the size of half-a-crown (a rule I have laid down in my manual if version is to be done at all, but which I decidedly object to)—if, however, under these circumstances *to a certain extent favourable*, we have a mortality of 1 in 3 cases to mother and child, what must be the prospect if attempted before, when the os will only admit the point of one finger, when it is unyielding of itself, and when the violence to effect what is aimed at must be extreme? It is no argument that an individual may have two or three cases without a death occurring, as it is just as probable that the next two or three may be all fatal; hence the error of advocating general principles of practice from a few solitary examples, however successful they may be. One thing is certain; if cases of placenta prævia are left to themselves, they are sure to end fatally to both the mother and child, except in those few rare cases where spontaneous separation has taken place, and where probably the adhesion had not been very extensive. Cases of this nature seem significantly intended by Providence to point out the true method to be adopted in all cases requiring manual interference.

Placental adhesion to the os is by some described as over its entire circumference. I am inclined to believe such cases to be far more rare than is generally admitted, and, even where it has so adhered, I have always found a weak point somewhere within the circle, from which (easily detected by a well-practised finger) the detachment should commence, and is effected without much difficulty. The old plan of boring through the placental mass with the fingers pointed to a cone, is barbarously rude and unnecessary; and will, if practised, certainly be followed by an

immediate increase of hæmorrhage, produce rapid exhaustion, and render future operations more difficult and dangerous.

Rigby states "that partial attachments over the os are more frequent in the early months, and entire attachments characteristic of the close of the period of gestation." If this be a fact (but of which I am not very certain) then it is only reasonable to infer that attachments so recently formed would be weaker in comparison, and, therefore, more easily detached at the points where the adhesions had been *latest formed*, and thus a great point is gained in favour of the principle of detachment.

In cases of placenta prævia, repeated examinations cannot be too severely condemned. The one examination to ascertain the facts of the case should, if possible, be followed by prompt and energetic means to check the hæmorrhage by detachment, and thus facilitate subsequent delivery by the efforts of nature only. If the hæmorrhage ceases on the detachment of the placenta, the delivery may with great propriety be waited for, and without danger. I have scarcely known an instance to the contrary. In reference to the old plan of version and immediate delivery, for which the detachment of the placenta is proposed as a substitute, there is in the first place the heavy rate of mortality of 1 in 3 to the mother and 1 in 2 to the child, increased considerably if those cases are treated separately. Where the turning and delivery are effected when the dilatation of the os uteri has only just begun, that is, only to admit the point of one finger, or to within the size of a shilling at most—a rate of mortality would be shown which I do not hesitate to place at near 50 per cent. to the mother, and almost universally fatal to the child. These figures have to be compared with 1 in 44 to the mother, and 1 in 5 to the child, where the placenta is detached. Secondly, there is the probability of means having been previously tried to check the hæmorrhage by rupturing the membranes, or giving ergot, or both; either of which will immensely increase the difficulties and dangers attendant on subsequent version. Thirdly, if the os uteri is but very slightly dilated and not disposed to do so, the violence done by attempting to turn is very serious and the difficulties of version increased. Fourthly, if the hæmorrhage occur before the period of utero-gestation is completed (the seventh month for instance), the os is certainly not prepared for *extensive* dilatation, and I do not believe it ought to be subjected to such extreme treatment. Fifthly, the danger of waiting for a sufficient state of dilatation to effect version judiciously (say to the size of half-a-crown, as generally admitted by the best authorities), and the great loss of blood in the interim, when the hæmorrhage can be at once checked and the waiting for natural expulsion justified, by the simple process of detachment. Sixthly, in almost

all cases the prostration is often so great that even the necessary efforts of version will in very many instances hasten death.

I am convinced from long experience, that the dangers of version and hæmorrhage may in a great measure be done away with by the simple detachment of the placenta. I have never known it to fail, nor do I believe it will ever fail, if the detachment is completely and properly effected. We have also the highest authorities for stating that the arrest of hæmorrhage is complete in 19 out of 20. If even then for the sake of argument we admit one failure in twenty cases, the results of detachment would stand immeasurably superior to the old (and I trust soon to add obsolete) system of version and immediate delivery. In spite of all these facts and statistics, however, we still find practitioners occasionally clinging to the old system; and so lately as February, 1861, there are recorded in the *Edinburgh Medical Journal* three cases of placenta prævia by Dr. L. Roberts of Manchester, who not only advocates version and immediate delivery, but insists on its being done when the os is not dilated to more than the size of a shilling, and in one case scarcely admitting more than the point of one finger. It is fortunate the number of cases is but three and that the writer's experience cannot be extensive, he being a very young man, or it is difficult to say how and to what extent such opinions might influence the question publicly. I do think, moreover, that these cases should have been sanctioned by the colleagues of the author, in the hospital where the cases occurred, before being published, as they in some measure compromise the practice of that hospital generally, although we are aware that some of its senior officers have advocated opinions the very opposite to those of the author. As for the cases alluded to, and published in the *Edinburgh Journal of Medicine*—in the *first* the version was effected without much additional hæmorrhage, the child dead, moderately quick recovery, the os dilated to the size of a shilling: in the *second*, the os only admitting the point of one finger and situated high up; still he says the vertex of the head was felt (which I should imagine very difficult with so small a dilatation, and with the placental cushion between the finger and the head). Further on he states that a portion of the placenta was protruding (probably partially if not wholly spontaneously detached). Version was effected with little hæmorrhage, but secondary *post partum* hæmorrhage occurred nine days after, followed by a very slow and tedious recovery, child dead, as it is stated the pulsation of the cord had ceased. In the *third* case the os was rigid, about the size of a shilling; the funis had ceased to pulsate; turned and delivered; made a very slow recovery. The author concludes by observing that turning may be performed with safety to the mother, although the os is not dilated

to more than the size of a shilling; and he prefers it to waiting for the usually admitted rule of the size of half-a-crown and dilatable.

On looking at these three cases I see nothing to commend, but much to condemn. Two women make very slow recoveries and three children sacrificed, are not much to boast of. Two of the females are admitted to be three or four months in recovering from the effects of the practice. The general sentiments of the profession are decidedly opposed to such doctrines, and even those who have not as yet acceded fully to the merits of detaching the placenta, as a substitute for version and immediate delivery, still insist as a rule, that the os shall be dilated to the size of half-a-crown and be dilatable of itself, before turning is attempted.

Messrs. Harvey and McClintock say, "Turning in placenta prævia should never be performed until the os is so far dilated as to offer no material resistance;" and Dr. R. Lee says, "To attempt to deliver by turning in placenta prævia before the os is so far dilated that you can easily introduce the points of four fingers and a thumb; (however soft and relaxed it may be) is wrong; the dilatation must have proceeded so far, to insure safety and not entail injury on the os uteri." These sentiments are echoed by the profession at large, and they are equally unanimous in condemning every attempt at rudely forcing with great violence the hand through the uterine orifice in its early stages of dilatation, and when it is totally unfit for such violence.

It has been objected to detach at the seventh month, the os not being dilatable; but I have found it as easy dilatable at the seventh as at the ninth month, I mean *so far* as is necessary for detachment only, and therefore believe the objection to be more ideal than real. Then again, if it be an objection, how much more objectionable must it be to dilate more extensively as in version and delivery, where the violence must of necessity be greater? Others argue that the foetus may be retained for days after the detachment, and fever may arise, but as no facts have been advanced to confirm such an opinion, it can only be entertained as a mere supposition. I have never seen the foetus retained, and consequently no case of fever from such a cause has ever come under my notice, and certainly it does not apply to cases of foetus dead *in utero* from other causes. After all, there is such a remarkable difference in the statistics on these important questions, that I should consider any person reckless indeed of human life, who would advocate turning and delivery in cases of placenta prævia in preference to detaching the placenta, if at all aware of these results. But I think few could be found to go further than either of these doctrines, and advocate forcible dilatation at early periods, or attempt turning with the os only

capable of receiving the point of the forefinger, or at most dilated to the size of a shilling; increasing the already too great mortality, and adding greatly to the future difficulties to be contended with. One other point has been advanced—that detaching the placenta encourages indolence of practice, and screens inability. I would only observe in answer, that to watch such a case *as it ought to be watched*, is any thing but indolence; and to detach a placenta neatly and effectually, so as to realize our full expectations, requires as skilful and as careful manipulation as version. And if the accoucheur can accomplish the former, there is no fear of his being able to manage the latter.

In conclusion, I have never witnessed any bad consequences from detaching the placenta; there is infinitely less violence done, the danger is much reduced, future difficulties are of less importance, and the results far more favourable. And with the accumulated facts of the past forty years, from individuals of the highest standing in the profession, we may safely hope never so far to retrograde as to adopt the old and barbarous system of boring through the placenta, turning, and delivering the child. But even if we should be so far led astray as to accept this old barbarism, let us at least escape the opprobrium of attempting such practices in the earlier stages of dilatation of the os uteri, and *knowingly* increasing all the dangers attendant on such cases.

To conclude, we must not lose sight also of the many cases who, though they may not die from the immediate effects of version and delivery, nevertheless ultimately sink from the prostrating effects of hæmorrhage, months after their confinement, notwithstanding which they are considered as cures, and may have been recorded as such.

II.—*Illustrations of Puerperal Diseases*. By R. UVEDALE WEST, M.D., Fellow of the Royal College of Surgeons of Edinburgh, Vice-President of the Obstetrical Society of London, &c., &c.

(Continued from p. 12.)

FURTHER EXAMPLES OF THE PYOGENIC DIATHESIS: SALUTARY ELIMINATION OF A PURULENT *materiæ morbi*.

In Case II. the critical formation of small acute abscesses was an important element, conducting at once to a favourable result. That Case I. was not equally fortunate was due probably to the already too exhausted condition of the patient, as well as to the

circumstance that the deposition of pus took place in an important organ—the lung—which had for some weeks previously been affected with inflammation. The cases which will now follow illustrate still more fully and distinctly the doctrine of a favourable crisis resulting from the deposition and elimination of a purulent *materies morbi*, as well as they do the fact of the frequency of a pyogenic or pyæmic diathesis in pregnant and parturient women. It will, moreover, be seen from these, as well as from Cases I. and II., that while there is great capriciousness in the site selected by nature for the deposition of the pus to be eliminated from the system—for it may, as in Cases I. and V., or it may not, as in the other cases, be deposited in an organ or part whose disordered or inflamed condition had constituted the main feature of the disease—there is also considerable variety in the form of the puerperal malady which thus finds a natural remedy; the *pyogenic state*, as Cazeaux expresses it, not only manifesting itself in abdominal phlegmasies, but making its sad influence felt whatever be the disease attacking the puerperal woman. Thus, in Case II., one furnishing in itself an example of more than one form of disease—mania, pneumonia, tympanitis, &c.—the purulent deposit took place first in the calf of the leg, and finally in an abscess of the buttock; in Case VI., one of diarrhœa, the pus was deposited in an inguinal gland; in Case VII., one of pneumonia, in the calf of the leg as well as in the lung itself in the form of purulent expectoration; in Case VIII., one of dysentery with dysuria, in a cervical gland; in Cases IX. X. XI. XII. XIII. XV. and XVI. in the mamma; in Case XIV., one of peritoneal fever, in the mamma and on the skin in the form of a pustular eruption; in Case XVII., one also of peritoneal fever, in the lungs; in Cases XVIII. and XIX., under the fascia of the thigh—the two last being cases of strumous pyæmia *pur et simple*, that is to say, apart from the puerperal state, which was the exciting cause, there being no other disease present than the purulent collection itself.

That this capriciousness of purulent deposition is not peculiar to the puerperal forms of pyæmia was well proved to me by the cases of two male patients who came under my care a few years ago; in one of whom, after symptoms of severe acute articular rheumatism, a number of small abscesses formed in the neighbourhood of the affected joints. On these healing up, which they did very rapidly, a deposit took place in the kidneys, so that the patient had *pyuria* for several weeks, becoming very hectic and emaciated; this gradually ceasing, a large abscess of the liver developed itself, which being opened with the lancet, and kept discharging by means of tents for several months, the man got quite well and strong again. In the other, a purulent deposit in

an abscess of the testicle was suddenly transferred to the rectum, great quantities of pus being discharged with the stools. This man died.

The cases teach us further, that when this pyogenic state is present in any form of puerperal disease, we cannot *cure* the disease, we can only relieve the symptoms of it, and that imperfectly, until the purulent mischief which has been making its sad influence felt finds its way out of the system.

To sum up the argument, we may, I think, draw a distinction of some importance by bearing in mind that the *pyogenic* diathesis may exhibit itself in two very different ways; in the one, as a primary affection, suppurative inflammation of some organ—such as the lungs, the pleura, an articulation, the mamma—being at once set up, and along with a great amount of febrile disturbance constituting the puerperal disease; in the other, as a secondary affection coincident with some form of puerperal malady, and aggravating the symptoms of it, until pus being deposited in some, perhaps distant and unimportant part, its elimination from the system effects at once a complete cure.

In making these general remarks, which have been suggested to me by a study and comparison one with another of cases coming under my own immediate observation, I may be permitted to allude to the hideous possibility of the purulent deposit taking place in some important organ or part whence it cannot find an outlet, such as the brain or the cavity of the peritoneum or pericardium; but as I have never in my own practice met with satisfactory evidence of the selection by nature of any of these dangerous and necessarily fatal situations for such deposition, I cannot in this series adduce any cases in illustration of it; indeed, I think it is but seldom that nature makes such an awkward mistake.

CASE V.—PERITONEAL FEVER AND DIARRHŒA.

Mrs. T. J. of H., the mother of a large family, and a very delicate woman, was confined, after a very easy and natural labour, on the 15th of February, 1839. On the following day she had a rigor, followed by feverish symptoms. When I saw her she had headache, with delirium; the pulse 150, small and feeble; the skin dry; the tongue white; no pain in abdomen. I gave a calomel purge, and small doses of saline febrifuge medicine. The aperient dose acted powerfully. The next day the pulse was 130; there was less headache, but there was some abdominal pain, with tenderness on pressure. I ordered fomentations. The following day she was better, continuing the treatment. On the ninth day there was diarrhœa, with profuse yeasty evacuations, great pain, and return of fever; the pulse 120, small. I

gave a dose of castor oil, and after that, doses of chalk mixture, with laudanum and aromatic confection. The diarrhœa abated. Then for some weeks there ensued great debility, with shiverings, and what appeared to be an intermittent kind of fever, sometimes quotidian, sometimes quartan, with occasional diarrhœa and abdominal pain and tenderness; the pulse, which was sometimes so weak as to be scarcely perceptible, varying from 95 to 125. This condition was probably not so much an ague as a sign of a *pyogenic* state, such as I suspect was latent in Case III.; for, in the present case, the abdominal pain, which was felt in frequent paroxysms towards the decline of the disease, was several times followed by a purulent discharge from the vagina. Tonics, chiefly quinine, with ammonia and wine, gradually restored her from this state, but it was not until the end of the fifth week that she was able to sit up. I think, however, that it was chiefly to the purulent discharge from the vagina, which was probably a fortunate escape of the *materies morbi*, that we were indebted for the poor woman's escape. The lochia and milk were never *suppressed*, the former ceasing in due course, and the latter gradually drying up in consequence of the debility of the patient. There was at one time great irritability of stomach. There was generally great drowsiness during the fever fits.

CASE VI.—DIARRHŒA.

Mrs. J. R. of H., the wife of an agricultural labourer, a very robust, healthy-looking woman, aged 38, was confined of her first child on March the 17th, 1839. She had an easy labour. Diarrhœa, with griping and fever, came on in the course of the first week. I was sent for on the ninth day, and found, in addition, headache and sickness; the pulse, 120, small; white, slimy tongue, and great restlessness. I gave her a dose of castor oil, followed by carminatives, which gave great relief. The diarrhœa appeared to have been brought on in the first instance by an overdose of senna tea, taken on the third day. Some chalk mixture put a final check to the diarrhœa after it had continued about a week, and I discontinued my attendance. I was sent for again, however, about the 6th of June, and found my patient had relapsed into a nasty condition of hectic fever, with great emaciation, want of appetite, night-sweats, and other untoward symptoms. An inguinal gland, which had been inflamed a few days, I found in a state of suppuration, and lanced it; after which the patient rapidly recovered, taking a little wine occasionally.

CASE VII.—PNEUMONIA.

A. R. of Alford, a young, healthy, unmarried woman, was confined of a first child on February 23, 1854. She had a hard

labour, and had ergot of rye given to her by her medical attendant. On the fifth day of lying-in I was requested to see her, in the temporary absence from home of the practitioner who had attended the labour. I found her suffering from great fever, with headache, the pulse 140, small; she had plenty of milk, and the lochial discharge was not checked; the bowels were confined. I gave her 2 scruples of Dover's powder and an ounce of epsom salts in 8 ounces of camphor mixture, to be taken in 8 doses, one every four hours. I heard no more of the case, Mr. — having resumed his attendance, until 18th March, when I was called in on my own account. I found the patient suffering from great pain in the left side, and a cough with expectoration of dusky red sputa; the pulse 140, small and sharp; there was crepitation audible at the base of the left lung. I gave her a blister for the side, and calomel and opium in pills, to be taken with the following mixture—

R. Tinct. camph. comp.	
Spir. ætheris nitrici āā	℥ss.
Acid. nitric. dilut.	℥j.
Aquæ ad	Oss.
S. coch. ij 4tis horis, cum pilulâ.	

Under this treatment she speedily recovered from her pneumonic symptoms; the pain in the side abating, and the sputa improving in colour. The gums were never affected by the calomel taken. She had, however, diarrhoea and occasional vomitings, for which I gave her astringents and cordials. Her appetite improved. A few days later she began to complain of great pain in the calf of the right leg. At first I thought it would prove to be phlegmasia dolens, but presently the calf alone began to swell, and by the 1st of April it was manifest that an abscess would result. She had at this time a rapid, feeble pulse, 120, with hectic fever, emaciation and night-sweats. She had also some cough with purulent expectoration. The abscess on the calf was shortly after lanced, and she gradually got rid of all her untoward symptoms, taking cod-liver oil with wine and nourishment, but her convalescence was protracted. She has since had two more confinements, from which she made favourable recoveries, and is now in all respects a very healthy woman.

CASE VIII.—DYSENTERY; DYSURIA.

On the 9th of April, 1854, I was requested to visit Mrs. D. B. of Alford, the wife of a glazier, who expected her confinement

to take place in about two months. She was the mother of a large family. I found she had been suffering for nearly a week from troublesome diarrhoea. I gave her some ordinary medicine, viz., chalk mixture and laudanum; after which I entered in my note-book the following report in detail:—

April 10th—Dysenteric diarrhoea frequent all day, with great flatulence; the pulse 110, small and sharp. Gave acetate of lead in 2 gr. doses with $\frac{1}{4}$ gr. doses of opium and 5 gr. doses of compound galbanum pill.

11th—No improvement. Gave half an ounce of castor oil. At 6 p.m., finding that no urine had been passed since 11 a.m. the day before, I used the catheter. The pulse this day was 120, very small and sharp.

12th—The purging, gripings, and pain were rather better this morning, the stools having been more feculent since the castor oil was taken. The catheter was used morning and evening, and she went on with the medicine as given on the 10th.

13th—Purging and tenesmus, with great pain; passes her water slightly. The pulse in the morning 120, in the evening 140. Gave an enema of starch and laudanum, and the following pills in substitution for the acetate of lead doses—

R. Cupri sulphatis	gr. vj.
Pulv. opii.	gr. xij.
Cons. rosar. q. s. ut ft. pil. xij.	
i. 4tis horis sumenda.	

At 10 p.m., finding that a great accumulation in the bladder was aggravating the abdominal pain, I used the catheter, which gave great relief.

14th—9 a.m.—Fever; flushed cheeks; pulse 140; some perspiration; retention complete again. Catheter used. The pains this morning became those of labour, and at noon she was delivered of a small female foetus, which survived its birth about an hour. The diarrhoea was somewhat abated. 10 p.m.—Feels tolerable this evening; the pulse 140, thin; slight pains; very little purging; retention complete. The catheter drew off some scanty and high-coloured urine. Gave her some doses of liquor potassæ, with laudanum and compound tragacanth mucilage.

15th, 8 a.m.—Has rested well; only one evacuation in the course of last night; some pain in abdomen this morning; abdomen full; no tenderness; slight lochial discharge; is very flatulent; the pulse 160, soft; the respiration good; the tongue clean; complete retention of urine; catheter used; the urine

scanty as yesterday. *Same day, noon.*—Feels better; has got up a great deal of wind; the pulse 140, softer and fuller; some perspiration; diarrhœa returning.

R. Tinct. catechu.	℥ss.
Spir. ammon. aromat.	℥ij.
Tinct. opii	℥ij.
Spir. æth. nitric.	℥ss.
Aquæ menth. pip. ad	Oss. M.
Sumat coch. ij. tertiis horis.	

Same day, 6 p.m.—The diarrhœa continues; the pulse 145. At 9, catheterism again; the purging is becoming dysenteric as at first; added decoction of oak bark and logwood to the mixture, and gave some more doses of acetate of lead. The lochial discharge has entirely ceased.

16th, 8 a.m.—Three dysenteric purgings during last night; retention requiring catheter; the pulse 130. Continue medicine. *Same day, at 8 p.m.*—Pulse 125; there has been less pain throughout the day; diarrhœa and gripings frequent: 10 p.m., catheterism; urine more abundant and paler; feels better; abdomen less full; pulse 120.

17th—Pulse 125; pain relieved by a dose of morphia. At 10 p.m., has felt better all day; has passed a little urine; reappearance of lochia; pulse 125.

18th.—Much better; slight lochial discharge going on. Urine drawn off by catheter quite natural in quantity and appearance. The persistence of occasional diarrhœa made it advisable to continue the use of the copper, oak-bark, logwood, &c. She continued thus until the 21st, on which day I have this note:—

Pulse, a.m., 110; p.m., 98; diarrhœa four times to-day, with green vomitings. Chalk mixture with laudanum gave immediate relief. She went on, having occasional diarrhœa with irritable stomach until the 27th, when I found an absorbent gland inflamed at the side of the neck, with a feverish state of the system, the pulse 120. Suppuration took place a few days after, and thenceforward the progress to perfect recovery was rapid.

Suppuration of the breast, although most frequently occurring as a consequence of mismanaged lactation, being then what is called "milk-abscess," not unfrequently takes place as a favourable crisis in puerperal fevers. In other words, the purulent deposit takes place in the breast, and that in a large majority of cases, which is not to be wondered at, when we reflect on the power of sympathetic influence. I think the following cases tend to show this:—

CASE IX.—INTESTINAL FEVER.

Mrs. T. R. of L., a healthy woman, the wife of a small farmer, and already the mother of two children, was confined after a very easy labour on May 12th, 1848. I was sent for at the end of a week, and found the patient complaining of great pain in abdomen, chiefly at pit of stomach; there was a high state of fever, with headache, shiverings, white slimy tongue, and a throbbing pulse, 150 in the minute. No abdominal tenderness. One breast was hard and inflamed. I gave her in a pill three grains of opium with the like quantity of calomel, to be followed by an ounce of castor oil an hour after. I also gave her a saline effervescent mixture, and ordered the breast to be poulticed. The next day, the gastralgia was better, the tongue cleaner, and the pulse was 100 with diaphoresis. Subsequently diarrhoea came on, with great irritability of stomach, for the relief of which I gave her chalk mixture and effervescent to check the vomitings. The pulse did not again get higher than 100, although she continued very ill until the breast broke at the end of the third week. She had become quite emaciated. She had, after this, a very rapid recovery.

CASE X.—INTESTINAL FEVER; HYSTERIA.

Mrs. J. S. of H., a healthy young woman, the wife of a carpenter, was confined on July 12, 1851, after a hard labour with very rigid os uteri. It was her fourth confinement. Calling on the 18th July, I found she had just had a shivering fit. Her pulse was 150, small; there was much headache, with some hysterical delirium; the skin hot and rather moist; the lochia and milk abundant; the bowels had been moved twice the day before, but were now confined; the tongue white; no pain or tenderness of abdomen, except slightly in right groin. I gave her a draught with valerian, and the following:—

R. Magnesiae sulphatis.	ʒj.
Pulv. ipecac. comp.	ʒss.
Sp. ætheris nitrici.	ʒss.
Tinct. valer.	ʒij.
Mist. Camph.	ad Oss. M.
Sumat coch. ij. quartis horis.	

July 19th—Better altogether; less headache; bowels have been well moved. Continue medicine, without the sulphate of magnesia.

23rd—Saw her again this evening; she complains of being alternately hot and cold; she has hysterical urine; the pulse 140, soft and rather full; headache; white tongue; appetite is failing; lochia natural; milk checked; great excitability; pain in back, but none in abdomen.

R. Potassæ bicarb.	ʒij.
Pulv. ipecac. comp.	ʒss.
Sp. æth. nitrici.	ʒss.
Liq. ammon. acet.	ʒiiss.
Tinct. valer.	ʒss.
Mist. camph.	ad Oss. M.
S. coch. ij. quartis horis.	

24th—Is rather better; complains of pain in one breast.

26th—Is much better; the breast is suppurating; the pulse about 100. Continue medicine.

27th—The breast broke. After this she got well rapidly.

CASE XI.—INTESTINAL FEVER.

Mrs. J. S., of Alford, a delicate-looking young woman, the wife of a hedge carpenter, was confined on Feb. 13, 1853. She was already the mother of several children. Her present labour was an easy one, except that there was adhesion of the placenta, requiring manual extraction. On the fourth day she had some shiverings, followed by fever and headache, arrest of milk and lochia; she complained of "pain all over," and she had a small sharp pulse, 120; the bowels confined. I gave her my usual medicine, which, to save space, I will henceforth in these reports call *Mistura puerperalis*, viz.:—

R. Magnesiae sulphatis	ʒj.
Pulv. ipecac. comp.	ʒss.—ʒij.
Spir. ætheris nitrici.	ʒss.
Mist. camph.ad.	oss. M.

The next day she was better, the pulse had dropped to 90, and the following day it was down at 70. After this she was sometimes better and sometimes worse, until suppuration of one breast took place towards the end of her month. She then recovered at once.

CASE XII.—DIARRHŒA.

Mrs. T. S., of B. near Alford, a very delicate young woman, the wife of a farm-labourer, was confined of her first child, May 27, 1858. Her labour was an average one, but was followed by troublesome adhesion of placenta.

June 2nd—Two shivering fits, followed by fever and headache. Mist. puerp.

3rd—Pulse 105; tongue clean; headache; no abdominal tenderness, skin cool; lochia and milk all right. Continue.

5th—Pulse 100; no fever; diarrhoea; occasional pain in abdomen; tongue clean; lochia and milk abundant. Mist. cretæ cum tinct. opii.

6th—Feels better; pulse 86.

She was in this way better and worse until June 18, when I have the following note. A shivering fit yesterday; is now feverish; pulse 120, small; one breast inflamed. Mist. salina; poultices to breast.

A few days later the breast broke, and thenceforward she had an uninterrupted progress to perfect health.

CASE XIII.—DIARRHŒA.

The subject of the preceding case was confined again on August 15, 1859. On my arrival at 9 p.m., I found that the child had been born three hours, the labour having been very quick. The women present, thinking that they could get on "without the doctor," had been trying to remove the placenta by pulling at the funis, which they had torn away. There was great hæmorrhage going on; the poor woman was in great pain, and quite pulseless. I found the vagina full of large coagula. The placenta was still wholly within the uterus, the os being nearly closed; it was not adherent, and was easily removed. The funis, as I found, had broken off at about three inches distant from the placenta. I gave her a full dose of laudanum and some brandy, and left her about an hour after, with the pulse rallying.

The next morning I found her with a sharp pulse, 120, with a good deal of hæmorrhage and pain. I gave her another opiate.

The next day, August 17, in the evening she had a rigor, and on the 18th, p.m., I found her feverish; abdomen swollen and tympanitic, but bearing pressure pretty well everywhere except near left groin; she said she had not then so much pain as in the morning; the pulse 120, sharp; lochia checked; milk sufficient; bowels open. Mist. puerp.

19th, 20th, 21st—Better.

24th—Fever; pain; pulse 130, fluttering; diarrhoea; vomitings; headache; countenance flushed and anxious; milk in breasts; no lochia; respiration hurried; tongue clean. Chalk mixture, with laudanum and aromatic confection.

25th—Skin hot and feverish; diarrhoea better; abdomen soft and free from pain and tenderness; the pulse 130, firmer. Continue astringent medicine.

26th—Diarrhœa abated; is frequently sick; looks anxious; pulse 140.

R. Bismuthi trisnitratis ʒij.
 Acid. hydrocyanici. m. xij.
 Tinct. aurantii. ʒss.
 Mucilaginis acaciæ. ad oss.
 Sumat. coch. ij. quartis horis.

28th—Diarrhœa continues; pulse 120; the stomach now retains food; countenance and respiration better. Rep. mist. astring.

29th—Improving in every respect; pulse 108.

September 2d—Much better; diarrhœa well; pulse 96; complains, however, of one breast. Poultices to breast.

7th—The abscess broke, and I had no further anxiety about the case; for although, along with a copious discharge of pus from the breast, which continued for several weeks, there was also a troublesome cough with purulent expectoration, I regarded the latter as well as the former as merely a salutary outlet for an offending material, and not as *disease of the lungs*. And so it proved. And such in all probability would be the correct explanation of the purulent expectoration which took place in Case VII.

CASE XIV.—PERITONEAL FEVER.

Mrs. C. B. of T., near Alford, the wife of a farmer and the mother of three children, was confined on March 29th, 1859, after a very easy, natural labour. There was considerable hæmorrhage after the removal of the placenta, which was arrested by the usual means, such as emptying the uterus and vagina of coagula, cold dash, pressure, &c. On my visit on the third day, March 31st, I found her going wrong; the pulse 125, soft, moderately full, but unsteady, as if a rigor was impending; the countenance good; the after-pains still severe, as they had been all along; abdomen hard, full, and tender on pressure about left side; lochia abundant; hæmorrhage. She said that after I left on the 29th she had great hæmorrhage, with frequent faintness. Foment, and take mist. puerp.

April 1st.—Had a smart rigor yesterday soon after I left; nevertheless, slept well last night. This morning has much headache; white tongue; is thirsty; the pulse 120, with some sharpness; abdomen tympanitic; complains of nausea; bowels well relieved; lochia right; plenty of milk; skin cool; no hurry of respiration; pain and soreness of abdomen relieved. To take some effervescent saline medicine.

2nd.—Better; pulse 114; abdomen full, and very tympanitic; no tenderness; cheerful and smiling. Continue.

4th.—Pulse still quick, 110 ; abdomen not so full and tympanitic as on the 1st, but there is great tenderness on pressing fundus uteri, which may be felt distinctly ; bilious tongue ; appetite bad ; lochia and milk abundant. To take a few doses of quinine.

6th.—Pulse 104 ; feels altogether better ; soreness of abdomen nearly gone ; there is, however, still tenderness on left side, as well as more tympanitis than I like ; some headache ; tongue still bilious ; appetite not good ; skin cool and pleasant. Continue quinine, and take 4 grs. of pil. hyd. every night for three nights.

11th. Is improving. Continue quinine.

23rd.—Is nearly well.

But she was not quite well. About the end of the month a swelling appeared in one breast, which acquired the size of a walnut and broke, discharging a small quantity of pus. At the same time the skin became covered in several parts, about the arms, the side of the neck, the pit of the stomach, and elsewhere, with a pustular eruption, which was very troublesome, although decidedly salutary ; for after these purulent discharges she had no more unpleasant symptoms.

CASE XV.—INTESTINAL FEVER.

Mrs. G. H. of D., primipara, was confined, after an average labour, during which she had a dose of ergot of rye, on the 14th of May, 1859. I was summoned again on the 24th, and found the following symptoms:—Milk very abundant ; child refuses the breast ; fever ; pulse small and sharp, 130 ; countenance and respiration natural ; no abdominal tenderness ; lochia checked since yesterday, when she had a rigor—up to that time had been abundant ; bowels moved this morning ; headache ; appetite impaired. Mist. puerp.

May 25th—Feels better ; pulse fluttered, 110–120 ; no headache ; lochia returning ; child sucks again. Continue.

26th—Altogether better ; but having had another rigor, I gave her a few doses of quinine.

29th—Appetite better ; no more rigors ; less fever ; pulse 80. And so she went on, sometimes better sometimes worse, until—

June 4th—Left breast inflamed ; fevered, moist skin ; pulse 120, full ; appetite bad ; no milk in either breast ; lochia checked again. Mist. puerp., and foment the breast with scalded chamomile flowers.

9th—Breast suppurating ; frequent rigors. To take quinine again.

17th—The breast broke. After which, she had no unpleasant symptoms, although she had a slow recovery.

CASE XVI.—CONVULSIONS, MANIA, HYSTERIA.

Mrs. E. R. of Alford, the young wife of a carpenter—of a hysterical constitution, and whose mother had usually convulsions in her confinements—being in the fifth month of her first pregnancy, was attacked on the evening of March 23rd in the present year, with a violent fit of convulsions, and her medical attendant, Mr.—, was sent for. The convulsions having lasted all night, I was called in the next morning at 5 o'clock to see the case in consultation. The convulsions had ceased, but the patient was quite comatose, with dilated pupils disobedient to the light. Her pulse was rapid but soft, and she looked pale and œdematous. There was no urine forthcoming for our inspection. Mr.— had leeches to the temples. We applied some liquid blister to the same parts. As we sat watching her, she groaned two or three times as if expressive of abdominal or uterine pains. I suggested the possibility of premature labour coming on, but Mr.— assured me that he had found the os uteri, on examination, firmly closed. She continued in nearly the same state all day, and about 4 o'clock in the afternoon expelled a small putrid fœtus, after which she recovered her consciousness, and appeared to be going on well until the afternoon of the 27th of the month, the fourth day from the miscarriage, when I was suddenly summoned again to see her, in the temporary absence from home of Mr.—. I found her furiously maniacal, raving with great volubility about hell flames. I gave her a draught containing a grain and a half of acetate of morphia, which soon quieted her, and she fell asleep. By desire of Mr.—, who had returned home, I saw her again late in the evening in consultation. She was just beginning to talk again, but this time she was going to heaven instead of to hell, &c. Another half grain of the sedative quieted her again, and during the following seven days she had no return of mania; the slight hysterical symptoms, which from time to time gave warnings of a relapse, being promptly checked by half drachm doses of liq. opii. On the 3rd of April, it being just a week from the occurrence of the maniacal symptoms, one breast began to inflame, advancing rapidly to suppuration, so that Mr.—lanced it at the end of another week, after which all her threatening symptoms disappeared.

The above-related case, which affords a good illustration of the protean character of affections of the mind and brain when they occur in the puerperal state, would perhaps have appeared more fitly among my first group of cases if I had fortunately met with it soon enough. It, however, furnishes an equally good illustration of the behaviour of the pyogenic diathesis in a rare form of

puerperal disease and, therefore, comes in admirably at the close of my reports of cases of that class.

In Case VII. there was purulent expectoration with hectic fever prior to, or coincident with the final elimination of the offending material, by means of the abscess in the calf of the leg. In Case XIII. there was cough, with purulent expectoration, going on for several weeks while the breast was discharging pus. In both cases, doubtless, the purulent expectoration was subsidiary, as an eliminating process, to the evacuation of the *materies morbi*, from the leg in the former case, from the mamma in the latter; there was purulent disease *in*, not *of*, the lungs, as I have already observed. And I think we may regard the occurrence of a similar phenomenon in the following case, although it was unassisted by any other outlet for the evacuation of pus, as affording another instance of a condition usually regarded as highly dangerous to life, becoming a salutary process in the kind of disease under discussion:—

CASE XVII.—PERITONEAL FEVER; DIARRHŒA.

Mrs. S. A. of H., the wife of a shoemaker, a very delicate, phthisical-looking young woman, was confined of her first child on the 12th of December, 1852. Her labour was a very easy one. On the 16th I was summoned again:—Frequent rigors since midnight; hot moist skin; pulse 120, small; headache; great flow of milk; abdomen soft, but painful and tender to touch; bowels well moved yesterday; lochia abundant. To foment abdomen and take mist. puerp.

Dec. 17th—Feels better; less headache; less pain; no more rigors; pulse 120. Continue.

18th—Attack of pain in abdomen at 11 this morning. Pulse 120, as before. *Applicentur hirudines xij abdomini, et repetantur fots.* *Cont. med.*

19th—Better; no pain; diaphoresis; pulse 100.

20th—Better; pulse 100.

27th—Pain in micturition; pulse 100.

R. Tinct. opii	3j.
Spir. aetheris nitrici	3vj.
Tinct. buchu.	3iij.
Liq. potassæ	3j.
Mist. camph.	ad 3vj.

M. Sumat coch. ij. quartis horis.

This medicine gave relief, with the aid of fomentations, and I heard no more until—

Jan. 9th—When I have this note :—Shivering fit this morning, with fever and some pain in groin ; pulse 130 ; dry tongue ; lochia arrested.

R. Potassii iodidi	3ss.
Liq. potassæ	3j.
Liq. morph. acet.	3j.
Decoct. sarzæ comp.	ad oss. M.
M. Sumat. coch. ij. quartis horis, cum unâ pilularum seq.	
R. Hydr. chloridi	gr. viij.
Opii	gr. ij.
Micæ panis q. s. ut. fiant pilulæ viij.	

10th—Better ; no rigors this morning ; pulse 100.

11th—Rigor this morning. Pulse 120. To take two grains of quinine every four hours.

12th—Diarrhœa ; great pain ; pulse 110 ; lochia returned. To take chalk mixture with laudanum and aromatic confection, omitting the quinine.

13th—Diarrhœa better ; no return of rigors ; stomach irritable ; dry tongue ; pulse 108. To continue astringent medicine.

14th—Pulse 120. Great pain in groin. To take iodide of potassium again, and apply turpentine liniment to groin.

15th—Better.

16th—Frequent vomitings ; diarrhœa ; pulse 120. Cough, with very hectic countenance. Pain in groin better. To take astringent mixture.

And so the case went on. The expectoration from the cough soon became purulent and she had night sweats, and on the 20th she began to take cod-liver-oil, which, with quinine occasionally, she took for many months, until the cough, hectic fever, purulent expectoration, and other untoward symptoms simulating genuine phthisis, altogether left her. Her convalescence was very protracted. She was confined again, April 24th, 1854, after which confinement, also, she had a rather smart attack of purulent fever, coming on upon the third day and lasting about a week. She is now a strong healthy-looking woman, having had four more confinements from which she "got away" without any unfavourable symptoms.

The pyrogenic diathesis in the puerperal state may show itself in the development of the *chronic* form of abscess, as in the two well-marked examples given below ; the first of which (Case XVIII.) I should have regarded as merely the coincidence of a large strumous abscess with pregnancy, if the recurrence of a precisely similar condition in the same patient towards the termination of her next pregnancy (Case XIX.) had not unmistakably shown that the abscess was really a disease, of which the

pregnant or puerperal state was the exciting cause. Cazeaux, indeed, speaks of a *suppurating arthritis* as a form of puerperal fever.

CASE XVIII.—SUBFASCIAL ABSCESS OF THIGH.

Mrs. W. C., of S. near Alford, a strumous-looking young woman, the wife of a labourer, was confined on the 20th May, 1841, after a very easy primiparous labour which I attended, the case having been given up to me by the medical gentleman engaged, and who had been attending the patient for some weeks, for what had seemed to be diseased knee-joint. About six weeks before her confinement she began to complain of pain and swellings about the knee, which extended up the thigh. At the time of her delivery, the whole thigh was much swollen and very painful, and the poor woman was in a bad state of health, with great emaciation, quick pulse, and other symptoms of hectic fever. At the end of the lying-in month, there being fluctuation along the whole extent of the outside of the thigh, I opened the abscess, giving exit to a very large quantity of strumous pus. She had medicine and nourishment given her to support the strength, the thigh was bandaged, and she soon made a good recovery.

That this subfascial abscess depended as much on a *puerperal* pyogenic diathesis as on a strumous constitution was well proved by the fact that, with reference to this woman's next confinement, which took place July 9, 1843, I have the following brief note:—

CASE XIX.—SUBFASCIAL ABSCESS OF THIGH.

Inflammatory pain about the knee and thigh began about a month before her confinement. A large abscess formed as before under the fascia of the thigh, which I opened the day before she got her bed. She made a good recovery, as on the first occasion, although this time there was left a permanent lameness of the hip.

I attended this woman in her two next confinements, but she had no more abscesses. Since then I have lost sight of her.

It is worthy of remark, that diarrhoea was either a prominent symptom, or was itself the disease, in most of the cases above given, in which a natural cure took place by means of an escape of purulent matter. Is it not possible that a similar elimination of *materies morbi* in puerperal diarrhoeas may sometimes take place without being detected, the pus escaping from the rectum?

(To be continued.)

III.—*Report on a Species of Palsy, prevalent in part of the district of Allahabad, from the Use of Lathyrus Sativus or Kessaree Dāl, as an Article of Food.* By JAMES IRVING, M.D., Civil Surgeon of Allahabad.

In a report * dated 30th March, 1857, I drew attention to the prevalence of what I considered to be a species of palsy that prevailed in Barra, Pergunnah of the Allahabad district, and which was supposed to be caused by the people living on kessaree dāl, a vetch known to botanists as *Lathyrus sativus*. The present report refers to the same disease as it appears in Khyraghur, a Pergunnah of Allahabad situated on the other side of the Tons river, adjoining Barra.

On the 24th January of this year, I joined the camp of Mr. Court at Kutka on the banks of the Tons, and the same day we marched to Mejah, where the Tehseeldaree of Khyraghur is situated. Hitherto, we had passed through a fine country, with rich soil and bearing abundant crops. At Mejah, on inquiring for cases of gutteea, which is the name applied by the natives to this kind of palsy—I was told that there were a few examples; but the only one brought to me was a case of the ordinary palsy. Leaving Mejah next morning, we passed over a low range of rocky hills and came to Koraon. On the march, we observed much black marl soil, and found kessaree dāl growing in profusion. The ground, in many places round Koraon, is swampy; tanks abound, and the water of these the people drink. On the surface of the soil, in many places, an efflorescence of nitre was noticed. The following morning, we came to Lurreearee. The soil, as before, was of dark marl; kessaree was plentiful as a crop, and cripples were noticed in most of the villages we passed. In proceeding next day, a distance of six miles, to Keeree, we traversed a poorer-looking part of the country than we had hitherto done. The soil, as before, was marly, and there seemed to be hardly any other crop at this season, than the poisonous vetch. In many places there were large tracts of uncultivated land, covered with low jungle bushes. We observed many cripples. Next day we crossed the Tons, and in doing so, entered Pergunnah Barra. Bad as the soil of Khyraghur had gradually become, as we were approaching Barra, the change for the worse was very evident on entering the latter Pergunnah. The black marl soil crumbled in the hand, and the ground was everywhere split into deep fissures and large hollows, as if so

* The present report was recently presented to the government of the North West Provinces, and is reprinted by permission of the Honourable the Lieutenant-governor.

much of the subjacent soil had sunk down from the action of water. Cultivation was carried on to a trifling extent; but the crops seemed scanty, and consisted chiefly of kessaree dâl. We passed no village of any size. On the 30th of January we arrived at Barra. On the road thither, we passed many ruined and deserted villages, and observed the usual black, dry, cracked soil—the country abounding in tanks, kessaree, and lame villagers. Next day we proceeded to Kurma in Pergunnah Arail and found the soil and crops steadily improving as we advanced; and we saw no paralytics. The crops were not, as in Barra at this season, confined almost exclusively to kessaree and gram, but were varied by a plentiful admixture of barley and wheat.

Such is a sketch of the extent of my marches in Khyraghur and Barra, during the nine days that I was out in camp with Mr. Court in the past cold season. The appearance of the soil alone was sufficient to indicate what crops one might expect, and if it were of dark marl, one might be equally certain that kessaree or, as it is otherwise called, *Chuptooa Muttur*, would at this season be the prevailing crop; and that when one found this soil and this crop, one might look with a certainty of finding men crippled in their legs, in the surrounding villages, or in their neighbourhood. The people of Khyraghur and Barra appear to drink tank water. These tanks are very numerous all over this part of the country; so numerous, indeed, that they cannot be intended for holding drinking water only, but are probably intended to drain particular pieces of land, and also for purposes of irrigation in the hot weather. Wells appear to be comparatively rare, and in such as existed, the water was found close to the surface—that is to say, within eight or nine feet of it. In some parts of the country that we passed through, there is a stratum of stone a short distance beneath the soil, which would render well-sinking difficult and expensive; but this cannot be the only reason that wells are so scarce, for in places where there is no substratum of stone and where the water is close to the surface, we only met with tanks. From what has been said of the soil and water of Khyraghur, it may be inferred that in the rainy season the country must be almost entirely covered with water; and this I believe is the case.

The khureef crop in the portions of Khyraghur in which paralytics are found, consists of cotton, which is produced in considerable quantity; koodoo is also largely cultivated; while jawar and bujra are produced in comparatively small quantities. The principal crop at this season is a coarse, but very productive kind of rice, called *Sathee*. The rubbee crop consists of wheat and barley in small quantity. They do not come to perfection. In all parts of Khyraghur and Barra in which I have seen these

grains growing on the marl soil, they seemed to be stunted in the growth of the stalk, the heads were unproductive, and the grains were dark-coloured and small in size. As rubbee crops we have also gram, linseed, arhur dāl, and lastly and chiefly, kessaree dāl. In my former report it was stated, that kessaree formed the principal food of the people of Barra for a great part of the year, being the cheapest; that it required little or no cultivation—no labour or trouble to produce it; and that it grows on damp swampy soil that would bear no other crops. The ground merely requires to be lightly ploughed once, and the seed is then thrown in, when it speedily germinates, and comes to perfection without farther interference on the part of the cultivator. Mr. Court, in the letter which forwarded my former report to government, has stated that kessaree grows in all seasons, varying in luxuriousness of growth only according to the season, and affording the only certain provision of life in Barra. All this applies with equal force in the case of those parts of Khyraghur in which cases of paraplegia occur.

When out of the district every opportunity was taken to inquire into the amount of lameness in the various villages, and to ascertain from those affected the history of their cases, as well as the prevalent notions of the people generally as to the nature of a malady which disables so many, otherwise healthy men. The following table, compiled from returns which Mr. Court very kindly directed the Tehseeldar to draw out, exhibits the extent to which palsy prevails in Khyraghur.

TALOOKAH.	Population.	Number of Paralytics.	Percentage of Paralytics to Population.
Chowrassee,	6,760	81	0.43
Barokhur,	9,194	146	1.58
Kohrar,	10,120	109	1.07
Dyah,	15,722	251	1.59
Mara,	20,789	102	0.48
Khurkee,	8,215	250	3.04
Total,	70,800	889	1.25

The disease is thus much less prevalent than in Barra, in which Pergunnah out of a population of 56,649 as given by the Tehseeldar in January of this year, 2,221 were paralyzed, or 3.92 per cent. The table above shows clearly too that all the Talookahs of Khyraghur do not suffer in equal proportions; and in fact it would be found that cases of palsy in particular places, are in exact proportion to the quantity of kessaree dāl cultivated. My impression from what I saw is, that in the table the amount of palsy in Khyraghur is understated.

At Koraon I spoke with some forty or fifty cripples who had been assembled there from all the surrounding villages; many of them called their complaint rheumatism, and appeared to imagine that it was occasioned by a noxious wind (*khrah hawa*) that blows in the rains. Others spoke of deleterious properties of the water during the rains as the cause, or at all events, a cause of the affection. Every one who has spoken to natives about the origin or cause of any obscure disease, knows how prominent are the parts that bad wind and bad water play. By means of them and of heat (*gurmee*), they appear to account for all manner of diseases. Others spoke of their feeding on kessaree as the cause of their palsy. Their ideas were not at all clear as to the agency of bad wind, bad water, and kessaree dāl respectively, in producing the disease in any case; nor could I discover whether they considered that bad wind, or bad water alone, would give rise to palsy in one who did not use the poisonous grain. One man said one thing, and another the reverse. Here, as at Barra, many spoke of the suddenness of their seizure; and several said how they had been working in the fields during the rains, when they all at once felt pain in the loins, as if rheumatism had seized them. An old man asserted, and many others told me the same thing—viz., that the disease is only produced in particular years; the theory being that it is only in those years that the *khrah hawa* prevails. There would appear, however, to be no foundation for this statement; for of the cripples I examined at Koraon, the year that they stated they had become lame was too various to admit of the idea alluded to being true.

The following are given as examples of the cases seen at Koraon:—

1. Dussorblee Chumar, aged 35, inhabitant of Purrureea, has been lame five months. He was working in a rice kait during last rainy season, and had sat down, when suddenly he found that he was unable to raise himself. He felt no actual pain at the time, but now his knees and loins ache when he rises from a sitting position to the erect posture. He used to eat, and still eats kessaree.

2. Jowhair Koonbee, aged 50, an inhabitant of Chokundee village, has been lame for three years. He was working in the fields during the rains, where he became lame. He had no fever or other illness before. This all the paralytics that I have examined assert likewise. He used to eat kessaree, and still does so.

3. Emam Bux, Mussulman, aged 40, inhabitant of Koraon, states, that he was labouring in the field during the rains about three years ago, when he became lame all of a sudden, and has so remained. He used to live on kessaree, and still does so.

4. Shewnundun, aged 10 years, has been lame since last rains.

He was herding cattle in the rains when he became so. He, too, has always lived on kessaree.

5. Biron Lall, Brahmin, aged 60, has been lame for two years. He has always lived principally on kessaree, and still does so. This man stated that since he became paralyzed, he has felt pain in the loins, but nowhere else.

These short histories correspond exactly with those given in my previous report. The characteristics of the affection appear to be the generally sudden seizure of the patient; the time of seizure, the rainy season; the absence of any accompanying fever or other acute disease; the absence of pain except in the knees and loins—and that only when they attempt to walk. The affected do not appear to fall off much, if at all, in condition in consequence of the disease; nor, so far as I could judge, did the legs fall away as one would have expected. There seems to be no blunting of sensation. I pinched equally hard an arm and an affected leg of the same person in many different instances, but all said that they felt as much in the leg as in the arm. Biron Lall, whose case is given above, as well as several others, have told me that they feel cold from the loins downwards more acutely now than they did before they became lame. Others complained of a tingling sensation in the legs.

A peculiarity of the disease is, that it affects men more commonly than women. Thus, it appears from a return kindly furnished me by Mr. Court, that in Barra at the beginning of this year, of a population of 33,951 males, 2087 are paralyzed, or 6.11 per cent.; while of 22,658 females, only 134 or 0.59 per cent. are affected. According to this return, the total population of Barra in January was 56,649, of whom 2221 were paralyzed or 3.92 per cent.* The same fact is apparent in the following return from Khyraghur:—

Talookah.	Population.			Paralytics.			Ratio per cent. of Paralytics to Population.		
	Males.	Females.	Total.	Males.	Females.	Total.	Males.	Females.	Total.
Chowrassee,	3,533	3,227	6,760	26	5	31	0.73	0.15	0.43
Barokhur, ...	3,855	5,389	9,194	139	7	146	3.60	0.13	1.58
Kohrar,	5,171	4,949	10,120	107	2	109	2.06	0.04	1.07
Dyah,	8,973	6,749	15,722	238	13	251	2.65	0.19	1.59
Mara,	10,666	10,123	20,789	93	4	107	0.87	0.04	0.48
Khurkee,	4,098	4,117	8,215	239	11	250	5.83	0.26	8.04
Total,	36,296	22,504	70,800	842	42	884	2.31	0.18	1.25

* In a return given in my first report, the population of Barra in 1857 was stated to be—males, 32,827; females, 30,663; total, 63,490. Affected with palsy, 2028, or 3.19 per cent.

Mr. C. B. Thornhill was good enough to obtain for me a return of the paralytics in Chowkee Burgah, Pergunnah Mow, Zillah Banda. From this it appears that there are in this part of that district 2964 men, of whom 199 or 6·71 are paralyzed; and that of 2562 women, 11 or only 0·43 are affected.

Of this complaint I have found various traces in other parts of India, besides those given in my former report. In the part of Pergunnah Kurchunnah of the Allahabad district which joins on to Barra and possesses the same soil, we have a few cases.* I had hoped to give some interesting details regarding the prevalence of palsy in other parts of Banda. But after waiting a long time for a translation of papers ordered for me through Messrs. Thornhill and Mayne, I found them so evidently erroneous that I do not produce them here. For example, in a vernacular table which was furnished by a native official, there was a column in which to insert the usual food of the various villages. This was almost invariably stated to be wheat, barley, bajra, kodoo; and kessaree was very seldom mentioned, under the name of *peas*, although it was shown in another column that paralytics were common. Thinking this very strange, and suspecting that the whole of the return was a mere effort of imagination on the part of the official in question, I wrote to Peearree Mohun Bannerjee, deputy collector of Banda, on the subject, and found, as I anticipated I should do, that the dāl of kessaree is extensively grown in the Pergunnahs affected by the disease, but the people call it by the name of "*chotee muttur*." He adds—"It is much used by the poor man, and grows abundantly in the hilly tracts of the Pergunnahs Cheebo and Tirohan."

The disease would appear to prevail in the Mirzapore district also, but I have failed to obtain any information regarding it through the officials, although I tried to do so.† Captain F. Chapman of the Allahabad military police, who has lived in the interior of that district, told me that "cases of paralysis from kessaree dāl in the Mirzapore district are few; but I have never seen a case in which the slightest doubt was expressed as to its use having been the cause. Women, however, are never to my knowledge affected, only the males." He also states, that in the Mirzapore district the vetch is named *karow dāl*.

* There is not a trace of the disease to be found indigenous, in any other part of the Allahabad district. Mr. Court made full inquiries on this subject, and found it unknown in the Doab, or across the Ganges. Several Tehseeldars said that they had seen cases, but that they were among men who had come from Barra, Khyraghur, or Banda.

† Since this was written I have received a valuable communication from Mr. C. B. Dennison, collector of Mirzapore, showing that the disease prevails to a great extent in that district. I hope soon to make known the information above alluded to.

The same kind of palsy is also said to prevail in the neighbourhood of Patna and Gyah. Dr. Allen, the civil surgeon of the latter station, informs me that "it is very common in the villages of the district." He had not seen the disease himself, but was told by one of his native doctors, that "a man sleeping near kessaree dāl, which has been cut and collected in a heap in the time of harvest, may become affected with palsy in consequence." This is probably an oriental delusion; but it shows that the natives of that part of India are aware of some connection between palsy and kessaree as a cause thereof.

I have been informed, that the sepoy part of the army, which accompanied General Elphinstone in the first expedition to Cabul, suffered much in consequence of eating kessaree dāl. They well knew the deleterious effects produced by its constant use; but as they were often reduced to a pound of attah as their daily ration, they were very glad to make up the deficiency with the dāl; and in consequence of eating it, many men are said to have suffered from debility, by which term is probably indicated the same symptoms as are observed in Khyraghur and Barra.

I have asked several intelligent and educated Baboos as to the existence of the disease in Bengal, but as far as I can learn, it is not known. Kessaree dāl is found extensively in Bengal, but it is not employed as food to the almost total exclusion of all other grains, as in Barra and Khyraghur. It seems to be very generally eaten by way of a change, as it is also in the Punjab. There is a disease in the horse known in Bengal and other parts of India, which affects the spine in the loins, and appears to be of a paralytic nature. A horse so affected is said to be "gone in the loins." Now, it seems by no means improbable, as suggested by a writer in the *Englishman* newspaper in speaking of the disease among the natives of Barra, that this affection in the horse is similar, and that it is produced in the same way, namely, by the horse feeding on gram largely adulterated with kessaree dāl. I do not know the appearance of the gram usually sold in Bengal; but there is hardly a sample of it to be procured in many parts of the north-west provinces, which does not exhibit a considerable admixture of kessaree.

As to the nature of this affection, I believe it to be paralytic. Others, however, have thought that it was of the nature of rheumatism. Assistant-surgeon Abbott, of Rewah, who has seen cases of the disease, informs me that he inclines to this opinion, which is also not uncommon among the natives. The examination of a few bodies after death, might probably enable one to give better reasons than can at present be assigned, for maintaining either one side or the other. Hitherto, however, I have failed to obtain any cases for dissection. There can be no

doubt, that if what has been stated above on the authority of affected natives as to the history of the disease is true, there are circumstances that would appear at first sight to point to rheumatism as the essential part of the disease. For example, it is generally, if not always, contracted in the rainy season; and again, men who are more exposed to damp and other causes of rheumatism, are much more liable to the disease than women who are less exposed. This, however, does not bear investigation. We have the rains over the whole country, and men equally exposed to their influence everywhere; but the affection is not universal, being confined to certain parts in which the kessaree dâl is cultivated. There are, besides, other arguments against the supposition that the affection is rheumatic. If it were so, we might expect to find other parts occasionally affected, and not invariably the lower extremities only. Again, the only uneasiness complained of—it does not amount to pain—is in the loins, as one would expect in palsy of the lower limbs. But what seems to me to be the strongest argument in favour of the lameness of Khyraghur and Barra being of a paralytic, and not of a rheumatic nature is, that a similar affection has long been known in various parts of Europe, as well as in India, to result from the use of kessaree dâl, as well as of some other members of the natural order of plants to which the kessaree belongs—viz., the *Fabiaceæ*. In these cases, the disease has been regarded as paralytic. The affection, in fact, simply seems to be an instance of what is termed chronic poisoning, that is to say, injurious effects produced by frequently repeated doses of a poison. Other substances, in frequently-repeated small doses, produce palsy. Of this we have familiar instances in the case of mercury and lead; although in the latter instances the affection is more curable, as these poisons admit of elimination.

As to the treatment of palsy produced by the use of kessaree, I have nothing satisfactory to add. Five cases were recently under treatment in the government dispensary, and to a certain extent improved on being well fed; but when remedies were administered, they took fright and all left. Situated as the people of Khyraghur and Barra are at present, if any were cured in Allahabad and were sent back to their homes, they would again be exposed to the action of the exciting cause of their malady—as they would be compelled to live on kessaree—and would again, in all probability, become affected.

The form of paraplegia to which I have drawn attention, is one of great interest. As yet our information regarding it is meagre, although the subject seems well worthy of further and fuller investigation, with a view of mitigating the evil. It is indeed remarkable that thousands of people, who know that a

particular grain may render them lame, yet continue to use it as food. Is this because they must either eat the poison or starve? Will no other grain grow and be productive in the affected parts of Khyraghur and Barra, except kessaree? If not at present, will drainage or other means not render the soil capable of bearing other and less deleterious crops? Are there no means, in fact, of inducing the people to give up the use of the poisonous food? These are practical questions well worthy of solution.

IV.—*How is Syphilitic (or Specific) to be distinguished from Non-syphilitic (or Non-specific) Psoriasis?* By T. M'CALL ANDERSON, M.D., Physician to the Dispensary for Skin Diseases, Glasgow.

THERE is no more important point in the whole range of dermatology than the separation of syphilitic from non-syphilitic affections, and indeed one distinguished dermatologist almost goes the length of dividing skin diseases into two great classes—the syphilitic and the non-syphilitic; the former being under the influence of mercury, the latter of arsenic. Now, although this statement implies a good deal more than what the author alluded to intends, it still contains a rough outline of truth, and serves to increase the importance of the question which I now propose to answer. And let it be understood that I look upon psoriasis and lepra as one and the same affection; the latter differing from the former merely in the shape of the eruption, which is either in the form of circles or segments of circles, owing to the healing of the patches in the centre and to their peripheral extension.

The points to be attended to in arriving at a correct diagnosis are:—

- 1st. The extent of the eruption.
- 2nd. The size and shape of the patches.
- 3rd. The seat of the patches.
- 4th. The colour of the patches.
- 5th. The appearance of the scales.
- 6th. The duration of the eruption.
- 7th. The age of the patient.
- 8th. The sensations of the patient as regards itching.
- 9th. The occurrence of relapses.
- 10th. The origin of the disease.
- 11th. The concomitant symptoms.
- 12th. The effects of remedies.

1st. *The extent of the eruption.* — Syphilitic psoriasis is not usually very extensive, although the eruption may be much dis-

seminated. Non-syphilitic psoriasis *may* be very limited, and in first attacks often is so; but it may, and often does cover the greater portion of the cutaneous envelope, although it *always* leaves intervals of sound skin between the patches.

2nd. *The size and shape of the patches.*—The patches of the specific disease are *usually* small, and almost invariably either in the shape of little isolated spots about the size of split peas (*Psoriasis guttata*), or of circles or segments of circles of *small* size (*Psoriasis circinata*). The patches of the non-specific variety *may* be very small and often assume the guttated and circinate forms; but *usually*, when the disease is at all severe, many of them are very large and irregular in shape—although mingled with, and at the edges of these, small spots and circles or segments of circles are often observed. These circles are often of large size, being sometimes two or three inches in diameter.

3rd. *The seat of the patches.*—The non-syphilitic disease, although it *may* appear on any part of the body, attacks the *elbows and knees*, almost without exception; from which it follows that these are the parts first attacked on the outbreak of the eruption. Some dermatologists (Erasmus Wilson, for example) go the length of saying that if the eruption does not exist on the elbows or knees, and if it has never visited these regions, it is not (non-syphilitic) psoriasis. But this is not the case; for instances do now and then occur in which these regions are not, and never have been attacked. I saw an example of this a couple of months ago in which the eruption was limited to the skin covering the right scapula, the elbows and knees having never been affected.

The syphilitic disease *may* be met with on the elbows or knees, but this is only accidentally and exceptionally. It attacks by preference the inner rather than the outer aspect of the limbs. It is said, that psoriasis limited to the soles of the feet and palms of the hand (*Psoriasis palmaria et plantaria*), is always syphilitic. I believe such an eruption to be usually, but by no means always, of a specific nature; and moreover, I am quite convinced that many eruptions on the soles and palms are called psoriasis which have no title to such an appellation.

4th. *The colour of the patches.*—In both forms of the disease, the bright red colour which is met with in simple inflammations of the skin is wanting, the eruption having a dusky-red or coppery tint. The coppery colour, however, of the specific is usually, though not always, much more pronounced than that of the non-specific affection, and, as a general rule, it may be said that the former has a dusky-red, the latter a distinctly coppery tint. This difference in the colour is not so well marked at the commencement of the eruption, because in this, as in almost all other

forms of syphilitic eruption, the tint gets deeper, as the eruption becomes more chronic.

5th. *The appearance of the scales.*—Non-specific psoriasis is characterized by silvery-white, thick, imbricated scales, the thickness in some situations, as at the knees, being often very great (measuring sometimes as much as the third of an inch), but in these cases the colour is usually not quite so white. By scraping the surface a little, the silvery colour is more distinctly seen, because (amongst the lower and dirtier classes especially) particles of dust accumulate in great abundance upon the patches. The scales of specific psoriasis are usually much thinner and not so imbricated. Their colour is sometimes white and silvery, but oftener greyish, and occasionally very dark (*Psoriasis nigricans*).

6th. *The duration of the eruption.*—Syphilitic psoriasis may last many months or even one or two years, when no treatment whatever has been adopted, but the majority of cases are more recent when first seen; while non-syphilitic psoriasis may have existed five, ten, fifteen, twenty years, or even, on and off, for a whole lifetime. Thus, the last case of syphilitic psoriasis which occurred at the dispensary for skin diseases (in which I noted the duration of the eruption), had existed for several months when I first saw it; while the last case of non-syphilitic psoriasis had lasted for six or seven years; and the second case before this one had existed, on and off, for thirty years.

7th. *The age of the patient.*—In the majority of cases of non-specific psoriasis, the eruption first makes its appearance before the age of twenty (although many appear after that time), and instances are frequently met with in which it first manifests itself at the age of eight or nine years. Almost all cases of the specific disease, on the other hand, necessarily occur, in this country at least, after the age of puberty, and not *usually* before twenty, because children are little if at all exposed to the contagion of syphilis (I leave out of consideration entirely, cases of hereditary syphilis in which psoriasis does not form one of the manifestations): so that we can at least go the length of inferring that cases of psoriasis which have commenced before, or even some time after puberty, are pretty surely non-syphilitic.

8th. *The sensations of the patient as regards itching.*—Great difference of opinion seems to exist in the minds of dermatologists, as to whether non-syphilitic psoriasis is an itching affection or not. Monsieur Hardy, for instance, says that itching is *always* present, while Hcbra, Devergie, and others maintain that this is *never* the case, unless some other cutaneous eruption complicates the disease. Now, when such distinguished authorities disagree on such an easily decided question, we must of necessity leave the works of authors and study the book of nature. Adopting this course, I have arrived at the conclusion that *some*

cases of non-specific psoriasis are accompanied by no pruritus, *some* by most intolerable, and the great majority by *slight* itching *now and then*. Specific psoriasis, on the other hand, follows the law common to almost all the syphilides and is rarely, if ever, accompanied by itchy sensations.

9th. *The occurrence of relapses*.—The course of non-syphilitic psoriasis is a pretty uniform one, appearing usually at first in the winter season, and often disappearing in summer, to reappear either the next or some succeeding winter. As the disease becomes older, the relapses are more frequent, and each attack more severe than the one which preceded it, till at last, although the eruption is ameliorated in summer, it never disappears entirely. This at least is its usual course when not altered by treatment. On the other hand, when syphilitic psoriasis has once *completely* disappeared, *so that all trace of it is gone*, it does not usually return again, although, while the eruption exists, new patches may appear from time to time. It is often, however, followed by other forms of syphilitic eruption, as for example, by an outbreak of tubercles.

10th. *The origin of the disease*.—In the specific form of the affection every one knows that the system is contaminated by the inoculation of a poison, which is usually communicated to the affected person by his contracting a hard chancre from another who is similarly affected. We must, therefore, interrogate the patient, when he can generally, if he likes, recollect having had a hard chancre not very long before, say from six weeks to six months before the appearance of the eruption. It is, however, pretty generally acknowledged (and even by Ricord himself, I believe) that the secretions from secondary syphilitic eruptions are capable of communicating the poison of syphilis to a healthy individual. A very conclusive case in illustration of this is related by Rollet*—"Jules C., silk weaver, aged 25, had never been attacked by syphilis, when, on the 11th April, 1858, he was severely bitten on the upper lip by Louis B. The wounds of these bites remained open more than two months. On June 26, when he was admitted into hospital (l'Antiquaille at Lyons) the patient was examined by M. Rollet, who observed the following appearances:—On the upper lip in the wounded parts two indurated nodi of the size of a shilling. Each induration was slightly excoriated on its surface; a double large indolent adenitis occupied the submaxillary region. For some days previous there had been scabs in the hair, with alopecia, papulous erythema on the body, and mucous papulæ on the scrotum; nothing was seen on the penis. Inf. sarzæ, and two pills of the protoiodide of mercury daily, were prescribed, also baths of

corrosive sublimate. Patient discharged, July 8, incompletely cured. Jules C.'s wife presented no trace of syphilis; she was the mother of a child, whom she nursed, and who was in good health. Now what was Louis B.'s state? At the date of 10th April, 1857, this man had entered the hospital with an indurated chancre of the corona glandis; this chancre had healed in three weeks. Since that period mucous papulæ had appeared on the skin of the penis and on the scrotum, and subsequently an enlargement of the posterior cervical glands and alopecia had occurred. Louis B. at the time he seized his victim with his teeth, an act for which he was sentenced to six months' imprisonment, had syphilitic sores in the mouth; he openly confessed the fact, threatening Jules C. to give him the disease by biting him." M. Rollet points out as an incontrovertible fact, ascertained by Tavernier, Diday, and several other medical men well versed in syphilitic diseases, the existence of two indurated chancres on the lips of the victim, chancres communicated by Louis B., the author of the bite. He then shows Louis B., affected a year before with indurated chancre of the penis, subsequently presenting evident symptoms of secondary syphilis, and still having syphilitic sores on the mouth at the time he bit Jules C. Now, what could be the nature of these syphilitic sores? Louis B. having had, a year before, on the organs of generation a Hunterian chancre, followed by all its consequences, this syphilitic patient, if there ever was one, had he even exposed himself to what for another would have been certain contagion, was not liable to contract a second infectious chancre; infectious chancre being inoculable neither to the individual already affected with it, nor to any syphilitic subject.

So that, in the absence of all history of a chancre, we must remember the possibility of transmission of the poison from secondary sores, and we must also bear in mind the probability that the patient, either from motives of false modesty or for other reasons, is concealing the truth, or that the primary sore escaped his notice.

Non-syphilitic psoriasis is not contagious, but is often handed down from one generation to another. Of the many instances of the hereditary nature of this affection which have occurred at the dispensary for skin diseases, one only need be mentioned in illustration. This is the case of David White, aged 26, weaver, admitted March 18, 1861, labouring under the guttated form of the disease, which was most abundant on the buttocks, backs of the thighs, knees, legs, and elbows. It had first appeared 17 years previously, and patient stated that his father had psoriasis, and that, of a family of nine, one sister and one brother were similarly affected.

11th. *The concomitant symptoms.*—One remarkable circumstance with regard to non-syphilitic psoriasis is, that it occurs almost invariably in persons who are in the most robust health; and so necessary is this state of health to the flourishing of the eruption, that it tends to fade, or even disappears entirely, if from some cause or other the patient becomes emaciated or is attacked by an acute febrile disease. And it has further been pointed out by Hebra, that psoriasis and scrofula are antagonistic to one another, he having only once in his long experience met with a coincidence of these two maladies in the same person. The syphilitic patient, on the other hand, frequently shows symptoms of a cachectic state of system. And the psoriasis is usually accompanied by other syphilitic symptoms, such as erythematous and papular eruptions, condylomata, sore throat, &c. As an illustration of this point, the case of Edward M'Ewan may be taken, who came to the dispensary for skin diseases, May 20, 1861, with syphilitic psoriasis assuming the guttated and circinate forms, these being seated principally on the limbs, but not at all on the elbows or knees. The diagnosis of the syphilitic nature of this eruption was assisted by the concomitant symptoms, namely, slight trace of a chancre on the penis, chains of enlarged inguinal glands, deep fissures at the angles of the mouth, ulcers on the tongue and on the mucous membrane lining the lips and cheeks, papulæ on the scalp, and enlarged posterior cervical glands.

12th. *The effects of remedies.*—It must be known to all that mercury is a never-failing remedy for the removal of syphilitic psoriasis, an example of which is furnished by the case of Mrs. Davidson, aged 39, who consulted me on April 3, 1861, (the minutiae of the case I am unable to give, as I write from memory, not having taken notes of it at the time), with syphilitic psoriasis in the shape of very distinct little coppery circles, covered by white scales. None of these circles were more than an inch in diameter, and were situated principally on the extremities. After twelve calomel vapour baths, no trace of the eruption was left, except in the shape of very faint red circles—all the scales, the coppery tint, and the elevation above the surface having disappeared. She was told to continue the baths, and as she never returned there can be little doubt that she was completely cured. The vapour bath was extemporized according to the suggestion of Mr. Henry Lee. She was told to heat to redness half of a common brick, to place this in a common pan, the bottom of which contained a little boiling water. She was then to place the calomel powder in a thin layer (20 grains) upon the surface of the brick which was not in the water, to put the pan beneath a cane-bottomed chair, and to sit on the chair

enveloped in a blanket, which was drawn *tight* about the neck—to prevent the mercurial vapour getting into the mouth, causing sickness and salivation—but covering the body *loosely*, so as to allow the vapour to have free access to and to act upon the skin. The hot brick has the effect of keeping the water boiling, at the same time as it volatilizes the calomel.

Arsenic, on the other hand, is the remedy *par excellence* in the treatment of the non-specific form of the disease, and to which a large proportion of the cases yield. As an illustration of this may be taken the case of William Martin, which occurred at the Dispensary for Skin Diseases, under the care of Dr. Buchanan. This patient, a blacksmith, aged twenty-five, was admitted March 5, 1861, with psoriasis covering almost the whole body, though leaving intervals of sound skin between the patches. It was the most extensive eruption of the disease I ever saw, and had existed more or less for five months. The joints were uneasy and painful, owing to the deep fissures which existed, and the hands were quite covered with the eruption, and so stiff and painful that the patient was unable to open the dispensary door on the day of his admission. He was ordered five drops of Fowler's solution thrice daily, and the dose was gradually increased up to fifteen drops. The exhibition of this dose soon occasioned slight pain in the epigastrium, and the conjunctivæ became suffused, so that it was diminished to six drops three times daily. On the 19th April there was merely redness of the skin left in the sites of the previous patches, and on the 26th almost all trace of the disease had disappeared. The following table gives a brief *resumé* of the principal points discussed in reference to the diagnosis:—

DIAGNOSIS OF SYPHILITIC FROM NON-SYPHILITIC PSORIASIS.

SYPHILITIC PSORIASIS.

1. Eruption *not usually* very extensive.

2. Patches *usually* very small, and in shape of spots (size of a split pea), or of *small* circles or segments of circles (seldom more than an inch in diameter).

3. Eruption not usually on the elbows and knees; more on the inner than the outer aspect of limbs. When limited to soles or palms, usually syphilitic.

NON-SYPHILITIC PSORIASIS.

1. Eruption *often* very extensive.

2. Patches *often* very large and irregular. When circular, circles often two or three inches in diameter.

3. Eruption on any part of the body, but *almost* invariably on the *elbows and knees also*.

SYPHILITIC PSORIASIS.

4. Eruption usually of a distinctly coppery tint, after it has become chronic; sometimes very dark, even nearly black (*psoriasis nigricans*).

5. Scales thin; not so imbricated; often greyish.

6. May last months, or even one or two years, when no treatment employed.

7. Almost always commences after puberty, and usually after twenty.

8. Rarely if ever itchy.

9. A relapse not usual after *all trace* of the first eruption has *completely* disappeared.

10. Can often be traced to a hard chancre.

11. Patient *may* be cachectic, and concomitant symptoms detected; *e.g.* roseola syphilitica, lichen syphiliticus, condylomata, sore throat, alopecia, &c.

12. Removed almost invariably by mercury.

NON-SYPHILITIC PSORIASIS.

4. Patches of a dusky-red or light coppery colour.

5. Scales thick, imbricated, white, and silvery.

6. Often of five, ten, fifteen, twenty, or thirty years' duration, or even almost a whole lifetime.

7. Most cases (not all) commence before the age of twenty.

8. Sometimes not itchy; sometimes intolerably so; generally *slightly itchy now and then*.

9. Relapses are the rule, and are often very numerous.

10. Can often be traced back to hereditary transmission.

11. Patient in robust general health; bad health incompatible with the eruption, and the eruption is antagonistic to scrofula.

12. In the majority of cases, removed partially or entirely by arsenic.

I have thus endeavoured to state clearly and simply the differential diagnosis of these two forms of disease which resemble one another in so many particulars, but which differ very considerably when one comes to consider the minutiae—the circumstances attending their outbreak and subsidence, as well as their external configuration and appearance. It must be understood that the rules which I have laid down are not invariable, although I have expressed myself very guardedly with regard to many of them which are subject to numerous exceptions.

Did time permit I might enter upon the interesting question as to whether non-specific psoriasis is in reality to be traced back to the syphilitic poison after its transmission through at least one generation, as is maintained by Wilson and others; but I

must content myself at present by observing that this point has not yet been thoroughly investigated, and that the arguments in favour of, appear to me to be more than counterbalanced by the reasons against such a view.

V.—*On the Prevention of Pitting in Small Pox, by the Application of the Linamentum Aquæ Calcis and Cotton Wool.*
By JOSEPH BELL, M.D., Physician and Clinical Lecturer,
Glasgow Royal Infirmary, &c., &c.

THE unnecessary multiplication of remedies is an evil of such magnitude, that it is incumbent on any person by whom a new one is proposed to show that it is not only equal, but superior to those already in use.

It is on these grounds that I venture to bring under the notice of the profession, a new *method* of arresting the development of the variolous pustule, and preventing the subsequent pitting—a plan which I have found much more successful and more free from objections than any of those hitherto employed. It is simply *the application of cotton wool, saturated with the Linamentum Aquæ Calcis, and applied to the face, neck, &c., in the same manner as it is used in the treatment of burns.*

I have tried all the modern ectrotics, and found each more or less objectionable; for example:—

1. The puncture of the vesicle and application of the nitrate of silver, as recommended by Serres, Bretonneau, and Velpeau, caused much pain, increased the swelling of the face, and after all proved only partially successful in preventing pitting.

2. I tried the sulphur ointment recommended by Midivaine, and found any little benefit which arose from it more than counterbalanced by the very disgusting smell exhaled.

3. I have employed the mercurial ointment and plaster, lauded by Oliffe and others, with very considerable benefit; but it gives the patients such a disagreeable aspect, and at same time exposes them to the risk of salivation, that I have always used it with no small amount of hesitation. Again, when the emplastrum vigo was used, its presence was not only painful, but it also increased the irritation of the skin and augmented the swelling.

4. The tincture of iodine, as recommended by Dr. Crawford, I have found much less efficacious than the mercury; indeed, in my experience the application of the iodine seemed to have very little effect, either in preventing pitting or allaying the swelling.

5. The solution of collodion I found lessened the amount of pitting, but it very often increased the irritation of the skin, and consequently augmented the febrile disturbance. When it becomes dry it contracts, a circumstance which at first sight would seem to have a favourable influence on the congested vessels of the skin, by pressing as it were the blood out of them; but I found that this very pressure had the effect of acting as an irritant, and thereby increasing congestion. Again, to be of any use, the solution requires to be often repeated, because it soon breaks up and adheres to the skin in hard detached patches, the edges of which irritate the skin exceedingly.

6. Glycerine I found of little value, because it either ran off the face, or dried up very soon after it was applied, and it appeared to me to be very objectionable from the sharp smarting which it caused.

7. I have seen a solution of nitrate of silver and collodion in glycerine applied to the face, and undoubtedly it was very effective in preventing pitting; but it produced a most repulsive appearance, and caused considerable pain to the patient.

8. I have employed lard, oil, and the linamentum aquæ calcis, and found that if any of them were constantly applied, no irritation was produced, and that the pitting was very much modified. But as all these substances either run off the face soon or become dry, their benefit entirely depends on the assiduity with which their application is repeated.

Being favourably impressed with the effects of these, especially of the linamentum aquæ calcis, I was led to think that if it were applied on cotton wool, in the same manner as used in the treatment of burns, that the difficulty of keeping it applied might be obviated, and that the effect would be beneficial. Under these impressions, I applied it in several cases of confluent small-pox which were under treatment in the Infirmary, immediately before the termination of my duties as physician to the fever hospital in 1854, and the result was so successful that, on my re-appointment in 1858, I employed it in every case of confluent small-pox with decided success. In some of the most severe cases of the disease, not a single pit was found to have been formed on the parts to which it was applied. At the point of the nose and around the mouth, where the patients contrived always to remove the dressing to some extent, numerous deep pits took place, and contrasted strongly with the uniform smoothness of the cheeks and other parts over which the dressing was permitted to remain undisturbed.

But, besides the effectual prevention of pitting, the dressing secured another important result—it *prevented the swelling of the face*. This effect was most marked. In the severest confluent

cases, if applied early, the swelling never attained any great extent; and in cases in which great swelling had occurred prior to admission, the tumefaction of face soon subsided after the use of the dressing.

The febrile symptoms became considerably mitigated after its application; indeed this effect was so striking, that I have been induced to regard the swelling of the integuments of the face and scalp as being in a great measure the cause of the high febrile excitement which continues to exist in the confluent form of the disease; but whether or not, I state the fact that in cases of severe confluent small-pox in which the application was employed, the swelling of the face, and also the febrile excitement, were so slight as to arrest the attention in the most decided manner.

The mode of its application.—The linamentum aquæ calcis should be poured on a plate; then masses of cotton wool, answering in size and shape to the parts to which the dressing is to be used, should be dipped in the liniment, and applied in such a manner as to completely cover the face and neck, leaving apertures for the eyes, nostrils, and mouth. The cotton should be closely matted together, so as to allow no crevice to exist, and a large handkerchief should be tied over all, having holes cut in it so as to correspond with the apertures over eyes, nostrils, and mouth. The dressing should be allowed to remain until convalescence, and if it becomes accidentally detached at any part it should be immediately renewed.

Dr. Stokes in an excellent paper on the prevention of pitting in the *Dublin Journal* for February, 1859, speaks favourably of the advantages resulting from the linamentum aquæ calcis, but justly remarks that it rapidly becomes dry from the heat of the face. The use of the cotton wool, however, obviates this drawback, and at the same time completely secures the object which this distinguished physician states should be kept in view in the treatment, viz :—

1st. The exclusion of air.

2nd. The moderation of the local irritation.

3rd. The keeping of the parts in a permanently moist state, so as to prevent the drying and hardening of the scabs.

In order to effect these objects, Dr. Stokes has employed lintseed-meal poultices with decided success. I feel assured, however, that the three objects mentioned will be much better attained by the method which I have now recommended, and, I am also inclined to say, with much more comfort to the patient. The lintseed-meal poultice must cause uneasiness from its weight, and no small amount of discomfort from the smell which must be produced. There can be no question but that the cotton wool

will secure both the exclusion of air and light, keep the parts in a moist condition, and, as I have stated, will reduce the local irritation, as is proved by the rapid subsidence of the swelling of the face after the use of the dressing. The permanent nature of the application must be a great advantage over the frequency with which poultices require to be changed, as by this the air and light are often allowed to get access to the parts, and in this way detract from the value of any application which requires to be frequently changed.

The exclusion of air and light seems to be of the greatest moment; indeed it appears that if this can be accomplished by any plan, pitting will be prevented, or at least modified. It was on this principle that John of Gaddesden introduced the practice of blocking up the windows, and hanging scarlet cloth around the bed of the patient.* In the *American Journal of Medical Science* for 1832, Dr. Picton tells us that he had no cases of pitting from variola when the light was excluded from the room. In confirmation of these statements, I may be allowed to remind the reader of the interesting experiments made by Serres. He placed a glass capsule over small-pox pustules, so as to keep out air and light; he ascertained that the pustule was rendered abortive in proportion to the exclusion of both, and that when both were completely excluded, the pustule rapidly dried up and left not the slightest scar behind. The same writer affirms that on one occasion he had to treat a number of small-pox patients in a kind of dark cellar attached to La Pitié, and that very little pitting took place in these cases.

It seems, therefore, that the exclusion of air and light will completely prevent the pitting in variola. I believe that no applications will be found more likely to secure these conditions than the one which I now recommend. Certainly not one of the remedies hitherto proposed is so well calculated to do so, and, besides this it maintains a permanently moist state of the parts, and thus removes the local irritation. I have used it extensively, and have always found it successful. The numerous students who visited the small-pox wards in 1858 and 1859, and who had ample opportunities of scrutinizing its effects, can fully bear me out in these statements.

* It must have been on the principle of the exclusion of air and light that the various kinds of poultices employed long ago by English physicians proved useful. Some of these cataplasms were composed of truly disgusting materials, such as horse dung, &c. Ladies of these bygone days, however, preferred allowing their faces to be swathed for weeks in these abominable messes, rather than run the risk of having their beauty destroyed by hideous pits and scars—a fear as prevalent in the present day as then, and very properly so. To prevent any such disfiguration is both a justifiable and commendable practice.

The plan of treatment causes neither pain nor uneasiness to the patient. It is attended with no risk, and appears to me to be in every way preferable to other methods.

Some may object to the appearance of the patient's face swathed in cotton wool; but this is much less repulsive than when nitrate of silver, sulphur ointment, mercurial ointment, poultices, or iodine are employed, and I may add, than the naked face itself. Indeed, few appearances are more disagreeable than that of the face of a patient labouring under confluent small-pox.

In conclusion, I beg to recommend this plan of treatment to the notice of the profession, principally on the practical consideration that it is capable of—

1st, Preventing pitting.

2nd, Allaying the irritation of the skin.

3rd, Reducing the swelling.

4th, Preventing the swelling, if applied early.

5th, Reducing and preventing febrile excitement.

VI.—*Case of Double Renal Abscess*. By FRANCIS HENDERSON, M.D., Resident Clinical Clerk, Royal Infirmary, Glasgow.

CONSIDERABLE attention has of late been drawn to the subject of renal abscess—a lesion, the existence of which, though often difficult to determine during life, is yet of great interest to the practical physician, inasmuch as it seems to be of much more frequent occurrence than might be supposed from a perusal of the standard works.

If the diagnosis of renal abscess is difficult, and perhaps sometimes obscure—resulting from the situation of the organ and its wide-spread sympathies—it all the more demands an earnest attention, for, independently of the direct value of the diagnosis as a guide to treatment, its establishment would greatly assist the interpretation of ambiguous symptoms, and prevent us from treating diseases which had no real existence.

The circumstances in which renal abscess may occur, as well as the causes from which it originates, are various. It not uncommonly happens in pyæmia, but then its peculiar symptoms would probably be overwhelmed by those of the general disease, and the diagnosis, if made out, would be of no practical value. In other cases it might be associated with more prominent disease, its own symptoms so slight and vague as to be hardly observed among many others, and therefore their source overlooked.

Again, suppuration is sometimes a result of previous renal disease which had declared itself by marked symptoms, such as severe pain in the lumbar region, increased by pressure, extending down the ureter towards the bladder, and shooting down into the testicle and thigh, accompanied by scanty and high-coloured urine, or by hæmaturia and the passage of gravel. In any such case the appearance of a large quantity of pus in the urine, together with vomiting and the establishment of hectic, would warrant the conclusion that suppuration had taken place in the kidney.

But there is still another class of cases in which suppuration goes on for a time in the substance of the kidney, unassociated with any other organic mischief, and yet no symptoms are manifested so decided as seriously to alarm the patient or even his physician, far less to point out the local source of the malady. No doubt, in the progress of these cases towards a fatal issue, symptoms arise which excite grave apprehension, such as extreme emaciation and uncontrollable vomiting; but still it is quite possible that the cause may remain undiscovered to the end. It is to cases of this description that we wish mainly to direct attention. The following instance testifies to the occurrence of such cases, and affords illustration of some of the phenomena which they exhibit.

On the 10th of May, 1861, J. B., aged 30, Stirlingshire, was admitted into ward 3, under the care of Dr. Bell. He gave the following history of his case. From the end of last summer until two months ago, he had been subject to rigors, which occurred usually twice a week, although the periods of invasion were not perfectly regular. These rigors he had been told, and believed, were ague fits. They lasted about twenty minutes, and the three stages were strongly marked. They did not produce any permanent effect of which he was conscious, except debility, which had gradually increased up to the present time. It was his practice at the termination of the rigor to swallow two ounces of whisky, and immediately recommence work. For the last two months he had had no return of the rigors, but had lost all inclination for food, and for some time back had been subject to vomiting.

On admission, patient was exceedingly emaciated and very feeble, his face sallow, his eyes sunk, his lips dry, and his whole appearance suggestive of the last stage of phthisis.

He *complained* only of weakness. He had a slight cough, but almost no expectoration, and physical exploration of the chest failed to detect any decided signs of disease. Heart's action was easily excited, and he got breathless on making any effort. Pulse rapid, but feeble. Had no pain in the stomach nor in any

region of the abdomen, even when pressure was made. There was no fulness in loins, and no abdominal tumour or visceral enlargement discoverable. There was, however, frequent vomiting of a thin greenish fluid. He hardly took any food, but his thirst was urgent. Tongue, clean; no diarrhoea. Skin dry. There was no oedema of the extremities, and his intellect was clear.

On examination, the urine presented a milky appearance. On standing, a very abundant greenish-yellow deposit fell to the bottom of the glass, leaving the supernatant fluid pale and slightly opaque. The deposit, when treated with liq. potass., was entirely converted into a tenacious transparent jelly, and the microscope revealed pus corpuscles free from any crystalline deposit or mucous debris. The case was considered one of renal abscess. The patient was ordered four ounces of brandy in the day, and latterly he took one grain of opium twice daily.

On the 17th May he died exhausted.

Autopsy.—Both kidneys were discovered to be the seat of extensive suppuration. Neither had formed any morbid connection with the neighbouring parts, but were easily removed. The left was considerably smaller than in health, and was irregularly contracted externally. It contained several abscesses, some as large as a walnut, communicating with each other and with the pelvis of the kidney; but little of the cortical or medullary structure remained. Pus was found in the left ureter and in the bladder; the coats of the latter were not diseased. The right kidney was much less disorganized—it was larger than natural. In its interior were discovered numerous abscesses, some of large size, but not opening into the pelvis or calyces. They were situated at the base of the pyramids, involving both the cortical and tubular structure to a certain extent. The larger abscesses were surrounded by a thick rugged wall, encircled with a rim of redness. The tubular portion was greatly congested, and some patches of deep redness were observed in the cortical substance.

The central part of this kidney was much less affected, and seems, from the amount of urine excreted shortly before death, to have been able to discharge its functions. On making a section through it, numerous small round yellow spots were seen, about the size of mustard seeds. These were ascertained by the microscope to consist of pus.

There is little room for doubt that disease had existed in this man's kidney for about nine months. During the first seven months there were only two positive symptoms of disease of which the patient had any remembrance—the occurrence of rigors and debility. It was natural, and to a certain extent it

was accurate, to suppose that the latter symptom was produced by the former. But, as the result has proved, the error lay in considering these rigors to be the *disease*, and not merely a *symptom*. From their periodicity and their close resemblance in detail to ague paroxysms, together with the absence of other obvious sign of disease, it is not a matter of wonder that the case was at first considered one of intermittent fever.

Nevertheless, it was a mistaken diagnosis. It would be well therefore, if we would profit by past experience, to bear in mind that fits exhibiting the cold, hot, and sweating stages, and occurring periodically, even when associated with other morbid symptoms, do not *necessarily* result from the malarious poison.

On the other hand, if the fits are not perfectly regular in their return, if they continue to recur after specific treatment has been persevered in, and if the patient has never been in a malarious district, then there are strong grounds for the suspicion that they are dependent upon internal abscess, that they are *rigors* marking the occurrence or extension of some hidden suppuration. In such a case, it would be proper to examine minutely the condition of each organ, using every means of investigation which science has placed within our reach, not omitting diligent inquiry into all symptoms, however slight, which have existed or which do exist, and comparing carefully these symptoms, one with another. Some morbid trace may be thus discovered, which may guide us to a particular organ, and lead us to infer that it is probably the seat of internal abscess. But in many cases, the symptoms may be such as to warrant a much more decided opinion, and even enable us to pronounce a confident diagnosis. In the doubtful instances, we must patiently watch for decisive symptoms, which, if the case be abscess, are almost certain to appear before the issue, whether that be death or recovery. For it is a general law, to which, however, there are some exceptions, that pus when once formed cannot be absorbed, but must find vent; and as it makes its way towards some surface, it gives rise to symptoms, which, if not conclusive in themselves, at length become so when the abscess bursts externally, or its contents escape by one of the passages. Here are the main points of an illustrative case which occurred lately:—

A gentleman in a neighbouring county suffered from periodic rigors. He was supposed to have the ague, and was ordered quinine. However, the case did not yield to this treatment, and a physician in this city was consulted. This gentleman found the liver somewhat enlarged and painful to pressure. Taking this in connection with the history, he concluded that abscess existed in the liver. The case went on for some time, until one day a large quantity of pus was discharged by the

bowels, and eventually the patient got well. He was altogether about *four* months ill.

But now, turning from the consideration of internal abscess in general, let us examine the symptoms which led to belief that our patient was the subject of *renal* abscess.

From his very aspect, his great emaciation and exhaustion, he evidently laboured under *some* serious organic disease. In this light, the negative symptoms became of service by leading us from one organ to another in search of the lesion. In turn, the kidney was investigated, and the urine examined. Here the evidence was found which, when inquired into and compared with the concurrent symptoms and the previous rigors, established the diagnosis—*a very large quantity of pus was present in the urine.*

But the question arose—did the pus come from the kidney? That it almost certainly did, was manifest on the following grounds:—

1. The pus was not derived from the urethra, for it came with the urine only; besides the quantity was too great to admit of this supposition.

2. The bladder was not the source of the pus, for had it been owing to chronic cystitis or catarrh of the bladder, the discharge would have been thick and ropy, resulting from the admixture of a large quantity of mucus, and numerous crystals of the triple phosphate would have been entangled in it. But it was not ropy, and that it did not contain mucous corpuscles or epithelial debris in any amount, was proved by the effects of liquor potassæ upon the yellow sediment. It was “entirely converted into a tenacious transparent jelly.” This conclusion was strengthened by microscopic examination, which besides demonstrated the absence of triple phosphate or other crystalline deposit. In addition to the characters of the purulent urine, there were no vesical symptoms complained of.

On the other hand, here is the positive evidence that the pus was renal in origin — its great abundance, its uniform diffusion in the urine when passed; the ready and distinct separation of the sediment, unmixed with mucus or crystals; the character of the urine itself, which, though tolerable in quantity, was obviously depraved in quality, pointing at some renal mischief. Upon these considerations the diagnosis was based, and, on reflecting on the case, there seems to have been little ground for doubt, or room for error. The only condition that suggests itself as a source of fallacy, is that of an abscess in a neighbouring organ, opening into the kidney or bladder, and so discharging its contents along with the urine. Many such cases have occurred. This possibility ought not to be forgotten; and though perplexing, the error of pronouncing such a case renal abscess might be

avoided, if the patient had been under observation at the first, or if an accurate history of the case was attainable.

Many of the German pathologists believe that pus can be discharged by the kidney *vicariously*, and they support their views by reference to recorded cases of empyema disappearing contemporaneously with the discharge of purulent urine. If this were proved to occur, it would be necessary to admit a vicarious discharge of pus, as another and an important source of fallacy to which the diagnosis in our case was exposed. But there is no satisfactory proof that bodies the size of pus corpuscles can enter the walls of capillary vessels without breach of surface; and it was the opinion of the late Dr. Golding Bird, that the supposed cases of vicarious discharge of pus from the kidney were instances where the pus burrowed its way to the kidney, emptied itself through an ulcerated opening, and was discharged by the urine—the cases in short which have just been referred to. If this, then, were the only source of error, it must be admitted that the indications furnished by the urine as to the nature and seat of our patient's malady, were of great value. The existence of pus having the characters specified, and the previous rigors, mutually explained each other, and formed the main supports of the diagnosis. As to the cause of renal abscess in this case, not much can be said. It was pretty evident from the history, and determined by inspection, that it was not occasioned by a calculus in the pelvis of the kidney. It was clearly a chronic suppuration; but what gave rise to the inflammation of which this was the result, remains a question.

There is an interesting circumstance in the history of this patient's symptoms, which affords scope for speculation as to its cause. It will be observed that there had been no return of the rigors for two months previous to his admission. It seems strange that this evidence of constitutional sympathy should cease, when the general debility was greatest, and when it might be supposed that the system would all the more readily be affected by local disease. We would not attempt to explain this occurrence, but merely make a single observation regarding it. Dr. Bristowe* mentions the case of a patient who was attacked by tertian ague, as it was supposed, three months before his admission into St. Thomas' Hospital. It persisted for about two months. About the time of its disappearance the urine was diminished in quantity, became pale and milky, and deposited an abundant whitish sediment. In this case the rigors apparently ceased upon the occurrence of pus in the urine. In our case, it was not ascertained how long the urine had been purulent, but, from the condition of the left kidney, it must have been some time, and were it not

* London Medical Review for May.

for the condition of the right kidney, it might have been considered highly probable that the cessation of the rigors mark the time when pus appeared in the urine. And such an opinion would not merely have been theoretical, for it is in accordance with experience that the bursting of an abscess, and the free escape of its contents, are followed by a diminution of constitutional irritation. The pathological state of the right kidney, however, was somewhat at variance with this view. There were numerous small abscesses in its substance, some situated in patches of deep redness. It can hardly be supposed, therefore, that all these abscesses were of an earlier date than the stoppage of the rigors, two months before admission. And if not, it may be fairly asked, How did their formation not give rise to rigors also?

In conclusion, we would draw attention to another feature in this case worthy of note, as illustrating how small a portion of kidney is sufficient for the *necessary* amount of blood depuration. Only about a fourth part of one kidney was in working order, and yet there was no trace of oedema on the one hand, and no symptom of uræmia on the other.

Since the above was written, a case presenting many identical symptoms, and having a very similar history, has been admitted into ward 2 of the Royal Infirmary, under the care of Dr. Ritchie:—

M. I., aged 24, Pollokshaws. Patient considered herself pretty well, and was able to work in the mill until three weeks ago; but on minute inquiry it is ascertained that she has not enjoyed perfect health for a considerable time. During last winter she had frequent rigors, her whole body shook violently, and her teeth chattered; she does not recollect exactly how often, but they occurred nearly every night for about two months. During this time she generally had to rise twice in the course of the night to micturate. The urine was passed without pain, but she observed that it had a milky appearance. About the end of the cold weather she states that the shivering fits ceased. Subsequently, the urine became more abundant, though it still continued milky. She remained in this state, without other ailment, until three weeks since. She was then seized with a severe pain in the right renal region. This pain, which was accompanied with scanty and muddy urine, and with general uneasiness and vomiting, continued for eight days, when she had a rigor, followed by pain, shooting round anteriorly towards the umbilicus, and also downwards along the course of the ureter. Patient could not stand without much increase of the pain, but lay with her right leg drawn up in bed; any effort to extend it occasioned great pain. From this time her stomach became very irritable, everything she took she vomited.

On admission, June 5, patient is pale, but not emaciated. There is, however, some puffiness about the face, eyelids, and hands, but no œdema of the lower extremities. She complains much of sickness and frequent vomiting, which has continued unabated—and also of pain in the right loin.

On examination of the abdomen, a circumscribed deep-seated swelling is discovered in the upper part of the right lumbar region. This is painful to the touch. There is no fulness over the right renal region posteriorly, but pressure here increases the pain she suffers. She passes a considerable quantity of urine (fifteen ounces in the twenty-four hours), having the following characters:—Milky when passed; on repose, a dense homogeneous greenish layer collects at the bottom of the glass; the supernatant fluid is very pale, sp. gravity 1010, and contains albumen; by the microscope the deposit is seen to consist entirely of pus corpuscles, as is confirmed by the action of liq. potassæ and acetic acid; there is no crystalline sediment; though kept for several days, the urine did not become foetid.

Had this patient come into the hospital a month ago, her history and symptoms would have closely resembled the first case. The rigors, it is true, tended to the quotidian instead of the tertian type, and they only continued two months instead of seven; but the main difference lay in the general state. This patient considered herself in tolerable health, whereas the other was suffering under extreme exhaustion. This difference would have rendered this case still more likely to have been overlooked than the former. But the acute attack which supervened, three weeks before the admission of the patient, greatly lessened the danger of this oversight, by at once directing attention to the kidney, as the suffering organ. The severe pain in the loin, shooting down the ureter; the scanty and muddy urine; the vomiting and general disturbance give evidence that the attack was nephritis; and the rigor which followed in eight days throws additional light upon the antecedent symptoms.

When the patient came under observation, the notable symptoms were—the painful tumour in the situation of the right kidney, the vomiting, and the purulent urine. That the tumour is a distension of the kidney, from an abscess in its interior, seems pretty certain, when its position is considered in relation to the history and attendant symptoms. Moreover, the tumour is distinctly circumscribed at its lower margin. This feature, as well as the high position of the tumour in the abdomen, render it exceedingly improbable that it is psoas abscess—a condition suggested by the attitude of the thigh, and the pain excited by its extension. But these symptoms might be produced by the pain to which the motion of the psoas muscle gave

rise, without any inflammation of the muscle itself. Besides, whatever extension of disease may have occurred in consequence of the recent acute attack, it is nearly certain from the milky aspect which the urine has long presented, that the kidney was primarily affected. Whether both kidneys are the seat of abscess, is necessarily a matter of doubt. But from the low specific gravity of the urine, together with the threatening of anasarca that exists, it is to be feared that the left kidney is also structurally diseased.

Persistent vomiting is a symptom common to both these cases, and it is a very frequent sympathetic result of renal disorder. It is important as exceedingly distressing to the patient, and as tending much to shorten life. In this last case the hydrocyanic acid checked it for a time, but it has now recommenced.

The purulent urine does not require comment. Its characters have been already described. They are the same as those of the urine in the first case; and for similar reasons the pus is concluded to come from the kidney.

It will be interesting to watch the progress of this case, and to study new symptoms which may yet be developed, as bearing upon the diagnosis. If both kidneys are the subject of organic disease, it is only too likely that a time will soon arrive when an inspection will decide the real nature of the case.

VII.—*On the Laws of the Mutual Interpenetration of Fluids.*

By JOHN MURRAY, M.A., M.D.

No. 1.

Most fluids, even when under existing circumstances they do not enter into chemical combination, yet when brought into contact, interpenetrate one another, and ultimately, if the process be not interrupted, form a uniform mixture, though their specific gravities be very different, and perfect external rest be maintained. On this property of fluids, which is no less characteristic of them than any other of their physical properties, depend the formation and stability of all mechanical gaseous and liquid mixtures. It appears in the ascent of vapours from volatile bodies into air and other gases, and in the absorption by liquids of gases and of vapours of bodies differing from the absorbent.

Before entering on the consideration of these classes of phenomena, it is proper to observe that the following remarks refer strictly to the mutual interpenetration of distinct and mutually

stable fluids; that is, to fluids which do not, under the given circumstances, react chemically upon one another. We must commence then by defining exactly what is to be understood by fluids which form a mechanical mixture, as distinguished from those which react chemically upon each other. When a mixture of two substances exhibits properties distinct from those of either of its components, in other words, when a new substance is formed, the action so far is chemical. A mechanical mixture, on the other hand, possesses the *sum of the properties*, chemical and physical, of its constituents.

From what has already been said, it will appear that the subject of this paper includes four classes of phenomena:—

I. The mutual interpenetration of gases.

II. Evaporation, or the absorption of vapours by gases.

III. The mutual interpenetration of liquids.

IV. The absorption of gases by liquids.

I. *The mutual interpenetration of gases.*—Under this division I propose to treat chiefly of those elastic fluids which retain the gaseous form at ordinary temperatures, and under ordinary pressures. The vapours of volatile substances, such as alcohol, ether, &c., which more readily assume the solid form, will be more conveniently considered under Evaporation. It will be understood, however, that I do not propose this as a philosophical distinction between gases and vapours, for probably all elastic fluids may with equal propriety be considered either as vapours or gases. As there is reason to believe that all substances are capable of assuming the gaseous form at sufficient temperatures, so also is it probable that all gases may, under suitable circumstances, be made to assume the liquid or solid form. In distinguishing gases from vapours, as above, it is my object first to treat of the more simple law which applies to sensibly perfect gases, and subsequently to inquire what modifications, if any, must be introduced in the case of those nearer their point of liquefaction, and which deviate appreciably from Mariott's law.

Priestley was the first to show that, when two or more gases which by mere mixture do not form a chemical compound, have once diffused uniformly through one another, they never separate again into strata according to their specific gravities, for whatever length of time the mixture be left at rest. He, however, thought it probable that the mixture of these gases should be attributed, in part at least, to external disturbance, and was inclined to believe that if two gases were carefully placed in contact, the one of greater specific gravity would retain its position if originally placed on a lower level than the other.

With the view of determining this question, Dalton, a few years before the close of last century, undertook a series of

experiments, and from these it appeared that gases always mutually interpenetrate, however carefully they be exposed.* He also found that, for the gases upon which he experimented, a definite period elapsed before equilibrium was established; and that for the same gases the time varied with the volumes employed, and with the freedom of communication between them. It was not, however, until 1801 that Dalton published the well-known law which bears his name. Dissatisfied with the current explanation that permanence in gaseous mixture is due to an imperfect chemical combination, he had made in the previous year a great variety of experiments, showing the action to be altogether mechanical. The phenomena he proposed to explain by the supposition that the atoms of any given gas repel each other, but exert no force whatever upon those of any other gas, so that with respect to any other such fluid they may be regarded as not existing. Hence a gas may be regarded as a vacuum with respect to all other gases, and in a mixture of two gases each acts independently upon the containing vessel with its own proper elasticity, just as if the other were absent. In support of this view he adduced, in addition to his own, the experiments of Saussure, Volta, Lavoisier, Watt, and Berthollet, which showed approximately that vapours increase the tension of all gases equally, and by an amount equal to the tension of the same vapours *in vacuo*. Pictet† and De Luc‡ had further proved that the influence of a gas of any density on hygrometric phenomena, is chiefly to retard the phenomena which ultimately become nearly, if not quite the same as, *in vacuo*.

As an immediate deduction from this theory, Dalton inferred that in the spontaneous intermixture of two gases in contact, the volumes which interchange position are not necessarily of equal magnitude, but are in the case of each gas inversely proportional to the square root of the density of that gas. This remarkable law follows directly from the hypothesis, that one gas may be considered as a vacuum with respect to any other gas, by a direct application of the theory of the passage of fluids into a vacuum.

According to this theory, the velocity with which a fluid rushes into a vacuum is equal to that which a body would acquire in falling from a height equal to that of the column of fluid. In applying this principle in the case of gases, it is obvious that the height of the column must be taken as that of a homogeneous atmosphere of the particular gas under consideration. If then P denote the atmospheric pressure, per unit area, at the place where the experiment is made, w the weight at the same place of unit

* Manchester Memoirs. New series, vol. i.

† Essay on Fire.

‡ On Evaporation. Phil. Trans. 1792.

bulk of the gas considered, and H the height of a homogeneous atmosphere of the same gas; we have—

$$H = \frac{P}{w}$$

substituting this expression for H in the general formula for the velocity of falling bodies—

$$v = \sqrt{2 g H}$$

we obtain $v = \sqrt{\frac{2 g P}{w}}$ or $v = \sqrt{\frac{c}{w}}$, where c is constant for

all gases. Hence the velocity of a gas rushing into a vacuum is proportional to the reciprocal of the square root of the density of that gas, a result which has been experimentally verified by Mr. Graham.* Whence if gases act as *vacua* to one another, and if a vessel containing a quantity of a given gas be exposed to an atmosphere consisting of a different gas, the former will escape, and the latter enter with velocities inversely proportional to the square root of their densities respectively. The same law will also express the initial motion, when two limited volumes of gas are brought into mutual contact. But if these volumes do not bear to one another the ratio indicated by the law, subsequent interchange must obviously be modified by unequal mechanical pressure.

For more than a quarter of a century after the publication of Dalton's law, physicists generally admitted that vapours and gases deport themselves in the same manner in other elastic fluids as in *vacuo*, with this difference only, that in the former case equilibrium is slowly established, whilst in the latter it is produced almost instantaneously. Though the theory proposed in explanation met, as we shall have to notice, with violent opposition, yet it was not considered necessary to submit the law to rigid experimental examination, inasmuch as it seemed to follow naturally from the notions formed regarding the constitution of gaseous fluids, and which were developed by Laplace, Poisson, and others. Little progress, indeed, appears to have been made in this branch of science, though of great importance, especially in meteorology, until, in 1823, Döbereiner communicated a new impulse to its investigation by the announcement of a very curious experiment. Having had occasion to collect large quantities of hydrogen over a pneumatic trough, he accidentally made use of a cracked jar. After this jar had remained twelve hours in the trough, he was surprised to find that the water had risen in it to a height of one

* On the motion of gases. Phil. Trans., 1846.

and a half inches above the external level, and that in twenty-four hours the difference of levels had increased to two and two-third inches. On repeating the experiment with glass vessels of different forms a similar result was obtained, but when atmospheric air or oxygen was substituted for hydrogen, no change of volume was detected. These experiments appear to have been the first that illustrated the great force with which gases tend to interpenetrate one another. The phenomenon was not indeed at first referred to this, but continued unexplained, until Graham took up the subject and showed its true cause. Doebereiner suggested capillarity as the cause, and supposed that hydrogen only is attracted by, and escapes through the fissures, on account of the extreme smallness of its atoms.*

In 1827 Magnus renewed the investigation, and showed that the vapour of water and other gases deformed themselves in a similar manner, a result which is opposed to the explanation of Doebereiner.†

We are indebted, however, to Mr. Graham for a complete investigation of Dalton's law as applied to gases. His first experiments on this subject were published in 1829.‡ In these he first allowed each gas to escape from a bottle into the atmosphere through a narrow tube, placed in the lower or upper part of the vessel, according as the gas was lighter or heavier than air, so that its escape should not be assisted by gravity. The result was, that the same volume of different gases escaped in times which were exceedingly unequal; the lighter gases escaping more rapidly, and the heavier more slowly. Hence, he concluded that the rate of interpenetration of gases varies according to some function of their densities, "apparently inversely as the square root of their densities."

Secondly, he allowed a mixture of two gases to escape from a receiver by apertures of 0.12 and 0.07 in diameter. On determining the proportions of the gases remaining after a given time, he found that a certain proportion of each of the mixed gases, independently of the other and corresponding to its own rate of escape, had left the receiver, and not merely a certain portion of the mixture. Hence the spontaneous intermixture of two gases in contact takes place, not by the interchange of sensible volumes, but by that of indefinitely small volumes of the gases.

In describing these and the following experiments, Graham introduces a system of nomenclature which, in order to avoid circumlocution, it will be convenient occasionally to use. The mutual interpenetration of gas he terms diffusion; the relative

* *Annales de Chimie. et de Physique*, xxiv. p. 332.

† Poggendorff, vol. x. p. 153.

‡ *Quarterly Journal of Science*, September, 1829.

tendencies of different gases to interpenetrate, he calls their diffusibility; and that volume of a gas which interchanges with a unit volume of air, he terms the equivalent diffusion volume of that gas. In other words if a gas—hydrogen, for example—be brought into contact with air, the ratio of the volume of hydrogen which diffuses into the air to the volume of air which diffuses into the hydrogen, is the equivalent diffusion volume of hydrogen.

In 1831, Döbereiner's experiment, of which until then he had not heard, suggested to Graham a method of continuing the inquiry, by which very accurate measurements of diffusion volumes might be obtained.

He first showed, by repeating Döbereiner's experiment and varying the circumstances, that hydrogen never escapes outward by the fissure without a certain proportion of air returning inwards. This proportion, in a large number of experiments, oscillated about that indicated by Dalton's law. Accurate results could not, however, be expected from this arrangement. Any inequality between the external and internal levels of the water would result in a corresponding inequality of tension between the hydrogen and the atmosphere which must obviously complicate the phenomena of diffusion by the effects of mechanical pressure.

This difficulty, together with the variable character of the fissures, led to the adoption of an exceedingly simple, and at the same time accurate, instrument, which received the name of diffusion tube. Its construction was as follows:—Into a glass tube open at both ends, from six to fourteen inches in length and half an inch in diameter, a cylinder of wood of nearly the same diameter was introduced so as to occupy the whole length of the tube except about $\frac{1}{2}$ of an inch at one end. This space was filled with plaster of Paris of the usual consistence for casts; and when this had set, the cylinder was withdrawn, thus leaving a receiver closed by an immovable plug of stucco. After the plug had been dried in order to render it permeable to gases, the tube was finally graduated by means of mercury into hundredths of a cubic inch, the numbers increasing from above downwards.

In conducting experiments with this instrument, it was filled with gas in the ordinary way, over a pneumatic trough; with this variation only, that in order to avoid wetting the plug, the air was drawn off by a siphon, the short limb of which came within half an inch of the upper part of the tube. The tube was now filled with the gas to be experimented upon, then emptied by the siphon and filled up a second or third time with the same gas, until the proportion of residual air ceased to be appreciable.

The volume of gas in the diffusion tube being noted, the upper end was opened, and a mutual interchange commenced between

the inclosed gas and the external air. As the volume of contained gas bore no sensibly finite ratio to that of the atmosphere, the process would obviously continue uninterruptedly until the former escaped entirely, and was wholly replaced by air.

The following is a table of the results obtained after a laborious and careful investigation, including all the permanent gases whose physical and chemical properties rendered the experimental method applicable. These are arranged so as to exhibit a comparison between the diffusion volumes deduced from Dalton's law, and those obtained experimentally :*—

GAS.	Specific Gravity.	Reciprocal of Square Root of Specific Gravity.	Diffusion Vol. obtained Experimentally.
Hydrogen,.....	·06926	3·7994	3·83
Carburetted hydrogen, ...	·5596	1·3370	1·344
Olefiant gas,	·972	1·0140	1·0191
Carbonic oxide,.....	·9712	1·0147	1·0149
Nitrogen,	·97137	1·0145	1·0143
Oxygen,	1·0563	·9506	·9487
Sulphuretted hydrogen, ..	1·1805	·9204	·95
Nitrous oxide,	1·5204	·8110	·82
Carbonic acid,	1·5245	·8103	·812
Sulphurous acid,	2·222	·6708	·68

The numbers in the last two columns agree very closely. In most cases, indeed, the correspondence is as accurate as could be expected, particularly when the disturbances caused by absorption, both by the plug and fluid over which the experiments were made, are taken into account. Experiments were likewise made with cyanogen, chlorine, hydrochloric acid gas, and ammonia; but from these no exact results were obtained, though in every case they were compatible with the law.

The time required for complete diffusion varies, as might be expected, for different gases. The gradual nature of the process is well illustrated by the following results, which were obtained by exposing the gases successively in a hollow cylinder, communicating with the external air by means of a knee-shaped tube. The aperture of this tube was directed downwards when the gas was lighter than air, and upwards when heavier, so that no aid could be obtained from a difference of specific gravities. Of 100 volumes of gas there disappeared :†—

* The specific gravities in this table differ in some cases from those given by Graham in his original paper. More recent and accurate determinations by Regnault and others have been adopted, in cases where such exist.

† Graham, Quarterly Journal of Chemical Science, Sept., 1829.

Gas.	Specific Gravity.	In Four Hours.	In Ten Hours.
Hydrogen,	1	81·6	94·5
Light carburetted hydrogen,...	8	43·4	62·7
Ammonia,	8·5	41·4	59·6
Olefiant gas,	14	34·9	48·3
Carbonic acid,	22	31·6	47·0
Carbonic acid,	32	27·6	46·0
Sulphurous acid,	35·4	23·7	39·5

It will also be seen from this table that, as is indicated by the law, equilibrium is more rapidly approached in the case of light gases.

It may then be considered as established, within the limits of experimental error, that different permanent gases interpenetrate determinate given volumes inversely proportioned to the square root of their specific gravities respectively. In other words—it is shown that the mutual interpenetration of gases is effected as if each were a vacuum to all others, with this difference only, that instead of the process being sensibly instantaneous, a definite time is required to complete the process.*

It is probable, however, that when the density of the gases increases to such an extent as to cause a perceptible deviation from Mariotte's law, a corresponding modification of this law will be found necessary.

Several theories have been proposed in explanation of the phenomena of the mutual interpenetration of gas. These I shall now briefly consider.

The hypothesis upon which Dalton originally founded his law was, as we have already seen, that an elastic fluid may be regarded as a vacuum to all other elastic fluids. To this it may be replied:—

1. That the expansion which takes place ought to be attended by absorption of heat, which the most careful experiments have failed to detect.

* Bunsen (Ann. de Ch. et Phys. [3], xxxiv. 357, xxxvi. 5, xxxvii. 405) has arrived at a different conclusion. He finds, for example, the ratio of the diffusion coefficients of oxygen to hydrogen to be 1 to 3·345 by one experiment, and by another, 1 to 2·73, instead of 1 to 4. This discordance, which is too great to be accounted for by errors of observation, is to be attributed to the resistance offered by the porous diaphragms employed, so that the rates of diffusion approximate to the rates of transpiration.—(See Graham on Motion of Gases, Phil. Trans., 1851.) Mr. Graham (Abstr. Chem. Soc. Qu. Jour., vol. vi. p. 234) found the rates of transpiration of oxygen and hydrogen through a mass of stucco to be the same as through capillary tubes; viz., 1 volume of oxygen to 2·3 volumes of hydrogen. These experiments of Bunsen do not, then, affect the law.

2. That whereas, according to this hypothesis, diffusion ought to be completed almost instantaneously, experiment shows that many hours elapse before equilibrium is established.

With a view to obviate these objections, Dalton subsequently assumed that the atoms of different gases and vapours universally repel each other. The spontaneous diffusion he explained by supposing the atoms to be of different sizes, so that when they come into contact, the different spherules press upon each other unequally and produce currents until the whole has become uniformly mixed.

If, however, we calculate the size of the atoms according to Dalton's theory, but on more accurate data than he possessed, we find that all gases may be divided into a comparatively small number of classes, each including many gases. There are indeed just as many of these classes as there are different combining volumes; for combining volumes are supposed by the atomic theory to be composed of an equal number of atoms, the size of which is proportional to the volume. In the following table the division of gases according to this hypothesis is indicated:—

Gases.	Size of Atom,
1. S,	1
2. O, P, As, &c.,	3
3. H, N, Cl, &c.,	6
4. Si Cl ₃ , HgS, &c.,	9
5. H Cl, NH ₃ , PH ₃ , &c.,	12
6. (C ₂ , H Cl ₂ , O), &c.,	18
7. P Cl ₃ , &c.,	24

If then Dalton's second assumption were true, gases which belong to one and the same of these classes, and which therefore have atoms of the same size, ought not to mix.

Berthollet, Murray, and many others, regard the process as the result of the operation of weak chemical affinity, and the mixture they regard, therefore, as an imperfect chemical compound. Gaseous mixtures, however, possess none of the characters of a chemical compound:—

1. Every substance cannot combine with every other, and in those cases where chemical combination takes place, the rapidity with which that combination takes place is, *cæteris paribus*, proportional to the strength of affinity subsisting between the substances. On the contrary, all gases, whether simple or compound, mutually interpenetrate, and the rapidity with which the mixture takes place is not dependent on the chemical nature of the gases, but only on their specific gravities.

2. Chemical combination is always accompanied by the evolu-

tion of heat, a phenomenon which never attends the mixing of gases.

3. When two or more gases mix by diffusion, the volume of the mixture is equal to the sum of the volumes of the constituent gases. On the contrary, in cases where a recognized chemical combination takes place between two gases, a change of volume generally if not invariably occurs. For example, when two volumes of hydrogen combine with one of oxygen, the product is two volumes of watery vapour.

4. The index of refraction of a gaseous mixture is, according to Arago and Biot, exactly a mean between the coefficients of refraction of the constituent gases, which is not the case in real chemical combinations. Such, in a theoretical aspect, is the unsatisfactory position of the phenomena of gaseous diffusion. The law still remains unprovided for by molecular philosophy.

The applications of this property of gases in the economy of nature, are so obvious as to render a minute discussion of them unnecessary. I may, however, just advert to some of these:—Observation has shown the proportions of oxygen and nitrogen in atmospheric air to be constant, not only at the sea level, but to a height of nearly 22,000 feet above its surface. Were now the passive force which maintains this uniform condition of a mere mechanical mixture suspended, the gases would separate into two strata, oxygen occupying the lower position by virtue of its greater specific gravity. Under these circumstances, vegetable as well as animal life must speedily terminate. Even setting aside the maintenance of physiological functions, the energy of oxidation would be exalted to such a degree, as in a brief period to convert the surface of the earth into an arid desert. Further, if we suppose the law of active diffusive energy to be abrogated, the carbonic acid derived from various natural sources would not ascend and mix with the atmosphere, but descend in streams to be absorbed by the waters of the ocean. Hence plants, deprived of an essential element of their nutrition, would speedily perish, and with them all vitality. It is likewise chiefly by this property of gases, that exhalations from putrescent organic matter even more noxious than carbonic acid are prevented from accumulating, and are diffused uniformly throughout the atmosphere, sufficiently diluted to be harmless to animals, and in fit circumstances again to supply the wants of vegetable life.

Among the more special illustrations is a beautiful application of the law in explanation of the function of respiration, and which exemplifies not only the general property of diffusion, but likewise the purposes observed in some cases by inequality of diffusion equivalents. The whole capacity of the pulmonary air cavities may be roughly estimated at about three hundred

cubic inches, and of this volume of air only about twenty cubic inches, or one-fifteenth of the whole contents, are expelled in a natural respiration. This quantity, it is obvious, will be the twenty cubic inches contained in the larger bronchial tubes; for it is impossible that the slight mechanical pressure caused by the muscles of respiration should have much influence over the smaller tubes, or on the still more distant air cells. The only effect which this pressure can produce is to make the contents of the smaller cavities advance into those nearer the trachea, merely to recede into their original position on the next inspiration. The function of the muscles, then, is simply to bring fresh portions of atmospheric air into closer contact with the vitiated air in the lungs. The actual substitution of oxygen for carbonic acid is effected by diffusion.

Further, after a forced expiration, about 125 cubic inches of air remain in the lungs, which are not under the control of respiratory action, and can indeed scarcely be withdrawn by the air pump. This state of permanent distention must be highly useful, by exposing an extensive surface for respiratory action, and is accounted for by the law of diffusion. The return oxygen is not equal to the carbonic acid with which it interchanges, but is greater in the proportion of their diffusion volumes, or in the ratio of 95 to 81. Hence there follows a tendency to accumulation of gas on the side originally occupied by the carbonic acid, which is only limited by the elasticity of the coats of the air vessels, and the increased facility which their distended state affords of emptying themselves mechanically of part of their contents.*

* For further information on the functions of diffusion in respiration, see Valentin's "Annual Reports on Physiology" in Canstatt's Jahresbericht, 1843, *et seq.*, also Regnault and Reiset, Comptes Rendus, 1848.

Mr. Graham proposes to employ the law of gaseous diffusion in determining the specific gravities of permanent gases, a method which he thinks would insure greater accuracy than that usually followed. If the specific gravities of two gases be D and D^1 respectively, and if the volumes of the same gases interchanged by diffusion be respectively V and V^1 , we have by the law the following equation—

$$\frac{D^1}{D} = \frac{V^2}{V^1^2}$$

which determines either D or D^1 when the values of the other three quarters are known. If the first gas be atmospheric air, whose specific gravity is unity, the equation becomes.

$$D^1 = \left(\frac{V}{V^1} \right)^2$$

from which the specific gravity of the second gas may be determined when V and V^1 are known.

VII.—*Cases of Excision of Bursæ*. By JAMES MORTON, M.D.,
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Materia Medica in Anderson's University.

(Read before the Glasgow Southern Medical Society.)

IN the structure of the bodies of all animals, we meet with innumerable instances of the care which nature bestows on those defensive means which are found indispensable for the proper preservation of other parts, that may be said to be more important to the healthy existence and comfort of living beings. But, though these defensive organs may be beautiful, as they are in themselves, though they may be unerringly planned, and admirably adapted to the purposes they are intended to serve; yet, like all other portions of the same organism, they are themselves liable to diseased changes. These in many instances are easily and speedily cured, yet we meet with a smaller number of cases in which, so great is the change, so decided the departure from the normal condition, that even the structure of the part is altered, and the organ thus altered becomes an impediment in place of a defence, and materially interferes with the natural functions of the parts which it seems intended to protect and facilitate.

These remarks will be found to apply to those small bodies which we are in the habit of denominating *bursæ mucosæ*, and which are met with in various parts of the human body, so that they may be said to be present there in not inconsiderable numbers. In fact they have been divided into two classes, the division depending upon their situation, and therefore styled accordingly superficial or subcutaneous *bursæ*, and deep *bursæ*; and the latter division is again subdivided into deep vesicular and deep vaginal *bursæ*. These vaginal *bursæ* are invariably found in connection with tendons, and with the fibrous sheaths through which these tendons are found to pass. They are more complex than the vesicular, and form a double sac, a sort of shut sac, enveloping the tendons. They occur generally in the neighbourhood of ginglymoid articulations, and by far the largest and most interesting of these *bursæ* are found surrounding or attached to the flexor tendons of the wrist and ankle. They are of considerable size and extent for some distance along the tendons. The deep vesicular *bursæ* generally occur in the neighbourhood of the great articulations, as the hip, shoulder, knee, and ankle, but are not confined to the limbs, as they have been discovered in the head and neck. Near the greater articulations the larger *bursæ* are found, and smaller ones are frequently met with elsewhere. It is not our purpose at present to point out their situations or otherwise describe these; but as we are about to say something of

those bursæ which become inflamed and enlarged over or near the patella, it may be proper to mention the seat and extent of those connected with the knee-joint. Of these we find there are several. Immediately above the articulation, between the extensors and front of the thigh-bone, there is one which, in comparison to others, may be said to be extremely large, often extending several inches upwards, and still more remarkable for communicating in many instances with the synovial membrane of the knee-joint, a fact which has often been adduced in general anatomy as a proof of the identity in structure of the serous lining of the interior of bursal sacs with that which covers the articular cartilages on the ends of the bones. There is also a considerable one at the inner and lower part of the articulation, between the tibia and the tendons of the sartorius, gracilis, and semitendinosus; another posteriorly between the origins of the gastrocnemii and the bone, and one between the popliteus muscle and the joint; and, like the large one in front, these generally communicate with the synovial membrane of the joint. One is usually found to exist between the semi-membranosus and the internal lateral ligament; and another is invariably found between the upper border of the tibia and the ligamentum patellæ, immediately above its insertion into that bone.

Not unfrequently these deep bursæ become distended, inflamed, or otherwise affected by disease, and we have thought it prudent to mention them thus, that they may not for a moment be confounded with the superficial or subcutaneous bursæ, concerning whose management in disease it is our intention to offer a few remarks. The latter were first described or referred to by Beclard in his "Additions to the general anatomy of Bichât," and the one demanding most of the attention of surgeons is the large one placed between the skin and the ligamentum patellæ. There is one between the skin and fascia covering the great trochanter of the femur, and another between the skin and fascia over the olecranon. Though not so well developed as the above, there are others evidently of the same nature, and met with between the skin and fascia over the angle of the lower jaw, and upon the dorsum of the hand about the phalangeal and metacarpo-phalangeal articulations. These subcutaneous bursæ are best developed in those whose limbs are actively and habitually exercised; they are not so perfect, or rather, they are not so easily demonstrated in others. They were formerly confounded with cellular membrane in dissections, but they have been ascertained to have a distinct and definite structure, and the cellular or areolar tissue cannot be inflated from them. This variety of bursæ does not possess any channel of direct communication with articular cavities—a point of material consequence in regard to the safety of their excision, when that is required. They

would seem, however, to be developed under the skin in various instances; where it is exposed to pressure or friction, as in club-foot, the part upon which the weight of the patient's body is thrown becomes so protected. The same has been observed to occur over angular curvature of the spine, an instance of which has been mentioned by Sir Benjamin Brodie; and a similar circumstance has been noted on the stumps of amputated limbs, over the projecting part of an exostosis, or unusual prominence of bone, also between unusual bony protuberances and tendons, and between muscles and tendons. These superficial bursæ do not communicate with the joints, and may be safely excised when necessary.

When the bursa over the ligamentum patellæ becomes distended, enlarged, and thickened, it forms what has been called ganglion of the knee, or housemaid's knee, from its frequent occurrence in this class of domestic servants. Though the latter name is now very generally adopted, it is well known to occur in others who, from the nature of their occupation, subject the parts in this situation to much pressure and friction, such as paviors, sailors, railway-plate layers, &c.* The first symptoms are usually those of inflammation, such as pain, redness, and swelling, succeeded by effusion of serum, and afterwards of sero-purulent matter, resulting eventually in abscess. In the early stages, and in mild cases, the symptoms may be removed by rest, position, and fomentations, with or without leeches, according to the severity of the inflammatory symptoms, and if the pulse has been at all excited, there will be some propriety in employing antimonials. Where the symptoms are more of a chronic nature, sorbefacient remedies may be of service, such as iodine in the form of tincture, or iodine paste, as preferred by some, or mercurial ointment. I am in the habit of using the iodine stupe or fomentation, thinking a dilute solution more likely to be of service. Either plan has its advantages, and the former, viz. the stronger solutions, may also act as counter-irritants; they may even vesicate, and thus answer all the purposes of blisters, which are also not unfrequently employed, and with benefit. Very often, cases which at first present acute symptoms, go on to suppuration, and then it becomes absolutely necessary to treat

* Bunion is the name applied to a similar condition of the bursa over the ball of the great toe; but the neighbouring tissues also become enlarged and indurated, even the ends of the bones composing the joint become swollen, and by and by suffer a kind of partial dislocation, and this very circumstance keeps the bursa in a state of continually increasing irritation, for it becomes more and more pressed upon between the enlarged bones and the boot or shoe. Excision of the bursa is sometimes imperatively demanded in such circumstances, should milder treatment prove unavailing.

them as abscesses. Accordingly, incisions from an inch to an inch and a half in length may be made over the long axis of the patella, by which the matter will usually be evacuated. In the non-inflamed condition, some have recommended pressure, which may be continuous, or it may be sudden and powerful, so as to burst the sac and evacuate the serous fluid which the bursa contains into the areolar tissue adjacent. Subcutaneous incision with a tenotomy knife has also been practised, or a similar use of a needle with a cutting point, followed by the application of a compress and bandage. Puncturing with a grooved needle, and setons of various kinds, have also been proposed. Ganglion of the flexors of the wrist should not be rashly interfered with, and in such a case a free incision has been found to be safer than puncture.

In some cases, it would appear that the bursa enlarges without the occurrence of much, or any inflammation. At all events, if there be any, it subsides without being succeeded by the occurrence of suppuration—it may be under the employment of the ordinary milder means of treatment; yet a state of chronic enlargement is produced, its walls become thickened, besides being distended, and the cause or causes being, from the nature of the occupation of the parties, necessarily continued, the distention and thickening go on *pari passu*, till the bursa presents the appearance of a huge wen, or rather of a firm but elastic tumour. This swelling may not be painful of itself, except when rudely touched, but it may very seriously impede the motions of the joint, and thus become a cause of disability in regard to labour, and a source of continual annoyance and discomfort. In such cases excision would seem to be demanded, and it is in such only that I would recommend it; and I would do so in preference to the seton, excision being in my opinion the safer and more merciful plan, promising a less painful and much speedier cure, a result which has been happily exemplified in the two following cases:—

Case I.—Isabella H., aged 28, admitted December 23, 1859. Patient, from the nature of her employment, is accustomed to be a great portion of her time on her knees, engaged in scrubbing floors in the infirmary. Nine months ago she was troubled with acute inflammation and swelling of the bursa over patella, which has since subsided into a chronic state. About a month ago she observed a small tumour in front of patella, which gradually enlarged, and was unattended by pain. On examination of knee, a tumour about the size of two closed fists is seen in front of patella, and completely surrounding it. The tumour feels fluctuant and movable, and is now a great inconvenience to her, though the joint itself is unaffected in most of its motions.

Several cicatrices of former varicose ulcers exist at and around ankles.

26th—To-day, was submitted to consultation, when it was agreed that an operation should be performed.

28th—This morning, she was put under the influence of chloroform, and the tumour dissected out. After excision, it was found to be a cyst containing thin serous fluid of a brownish-yellow colour; the walls of the cyst were nearly a quarter of an inch thick. Little hæmorrhage occurred during the operation; a few sutures of metallic wire were used to bring the integuments together, and water-dressing, with a bandage, was applied. Limb to be kept straight on a wedge pillow.

R. Pil. opii (gr. j). viii.,

One as required.

Hab. vini rub. ℥iv.: not to be continued.

Jan. 2nd, 1860.—The central piece of the flap, to the size of a half crown, has sloughed, and suppuration has commenced.

6th—Metallic sutures removed, edges are adhering; still some suppuration.

19th—Very little matter is now oozing from the wound; edges adhering, except at one small point; the portion that sloughed is now nearly well. A few days after this patient resumed her duties in the infirmary, as a servant, where she now is.

Case II.—John K., aged 50, admitted into ward 15 of the Glasgow Royal Infirmary, on the 7th February, 1861. From the nature of the patient's occupation, he requires while at work to be constantly leaning on the right knee, which about four years ago became very painful, and began to swell anteriorly. This swelling has been gradually increasing up to the present time, and causes him considerable difficulty in walking. On examination, a hard, circumscribed, elastic swelling, about the size of a large orange, is felt over the lower edge of the patella, which can be raised and freely moved from the surrounding tissues. No pain is complained of on manipulating the tumour, or moving the patella in a lateral direction. General health good.

To-day, after a consultation, the tumour was excised by making a straight incision over it and dissecting it out entire. The lips of the wound were brought together by wire sutures and strips of adhesive plaster. Wound found to have united by first intention; a small quantity of pus can be pressed out from the lower end. In the beginning of March he was dismissed well.

After excision a section was made of the globular mass, and it presented appearances very much resembling those mentioned

in the preceding case; the walls of the cyst were about the same thickness, and the serous fluid of the same brownish-yellow colour. The only circumstance worthy of remark was that one part of the cyst, and that the thickest, and from its situation most exposed to pressure and other sources of irritation, presented an appearance indicating the commencement of some kind of degeneration in its structure. I regret that this was not otherwise ascertained, or at least more carefully and completely examined microscopically, as, if true, it furnishes a strong argument for excision, and in opposition to setons in similar instances.

IX.—*Some Observations on the Sensibility of the Eye to Colour.*

By JOHN ZACHARIAH LAURENCE, F.R.C.S., M.B., London.
Surgeon to the South London Ophthalmic Hospital.*

IF, closing one eye—say the right—any highly luminous white ground, such as some portions of the sky on a sunny day, is viewed with the left through a dark tube, so as to exclude all extraneous light, after a little the eye will begin to feel fatigued, and a librating circular smoky spectrum will be perceived at the end of the tube. When the tube is laid aside and both eyes are directed to the sky, a similar spectrum will be observed, projected, as it were, on the surface of the heavens, but much darker. But if after a time each eye is alternately opened and closed, a rose-coloured spectrum is seen with the left eye, a pale-green one with the right. These appearances are seen still better if, instead of the sky, a white screen is used as the plane of projection in the second part of the experiment. At first an almost black circular disc is seen; this becomes lighter and lighter, till it is finally succeeded in the left eye by a bright rose-colour disc, surrounded by a violet border; in the right eye by an equally bright green, with a rose border. These spectra sometimes appear as if upon the surface of the screen, sometimes, on the contrary, as if originating within the eyeball itself, and indeed may be even seen with both eyes closed. To see the above phenomena in all their intensity, a slightly different plan must be adopted. As the field of projection, a sheet of dead black paper in a dark room is to be used; the spectra then seen with

* Since writing this paper, my attention has been directed to a series of elaborate disquisitions by Brücke and Fechner in Poggendorf's *Annalen der Physik u. Chemie*, vols. lxxiv., l., and xlv., to which I beg to refer my readers.

either eye are the same, and their colours most splendid, both as regards brightness and tint. At first an emerald-green disc appears, surrounded by a narrow carmine, or perhaps, more accurately, magenta border; the magenta tint is then seen to encroach more and more upon the green, till the whole disc is of the former colour, surrounded by a bluish-violet border; this last, in its turn, invades the magenta, till the final spectrum is of one uniform indigo-violet colour.

The above is the general sequence of colours which I, and other persons whom I have asked to perform the experiment, have observed; but these are liable to exceptions. Occasionally, the librating spectrum observed at the end of the tube in the first part of the experiment, acquires a faint rose, green, or violet tint. Sometimes, I have seen the spectra of the right and left eyes, in the second part of the experiment, reversed as regards colour.

These facts appear to prove the following propositions:—

1. That colour sensations may be excited in the retina, or brain, altogether independently of any external colour-stimulus.
2. That as an *optical* analysis of white light may be effected by a prism, so with the eye we possess the power of effecting, what may be called, its *physiological* analysis.
3. The last proposition tends to the conclusion that white light consists of three fundamental colours—magenta, emerald-green, and indigo-violet—corroborating in a remarkable manner the opinions of Professor Maxwell and Dr. Young on the same subject.
4. That a colour sensation excited in one eye is generally felt in the other, although this latter has not been exposed to the influence of light in any part of the experiment; that, in a word, a very close sympathy exists in the two retinæ, of which the consensual action of the two irides is probably but a reflex nervous consequence.

I may here allude to a distinction in ocular spectra, which has, I believe, not been taken much account of by observers of these phenomena. Some spectra seem as if projected on the plane to which we direct the eye, and in that case appear, as I have found from numerous measurements, linearly magnified in proportion to the distance of the eye from the plane of projection. Other spectra, on the contrary, are perceived, so to say, in the eyeball itself, and are of a subjective nature. Independent of the differences of their apparent seats, the two classes of spectra present certain other well-defined distinctions. Projected spectra are only perceived with the eyes open, and are generally but faint in colour; while subjective ones may be seen with the eyes shut, and are always intense in colour. At the same time

I am disposed to ascribe the differences of colour, in a certain degree, to the diluting influence of extraneous light; for projected spectra are always seen more vivid in a dark room than in daylight.

The green spectrum observed on a sheet of white paper, after prolonged contemplation of a red wafer, has been commonly explained thus—"When the eye has been for some time fixed on the *red* wafer, the part of the retina occupied by the red image is deadened by its continued action, and insensible to the red rays which form part of the white light from the paper; consequently will see the paper of that colour which arises from all the rays in the white light of the paper, but the red; that is, of a *bluish-green* colour, which is therefore the true complementary colour of the *red* wafer.*

That this explanation is not correct seems to me to be proved by the following experiment:—

I, at night, made a room (which is provided with thick American-leather blinds for ophthalmoscopic purposes), to all appearances, absolutely dark, then viewed with the left eye a small aperture in a dark box covered with a piece of emerald-green glass, behind which was the nearly white flame of a lamp. The right eye was kept closed, and covered with a thick handkerchief. After a time I blew out the light in the box, and looked at a screen covered with a sheet of dead-black paper. With the left eye a large carmine-coloured projected spectrum of the flame could be seen; with the right eye I generally perceived no spectrum at all, or if any, but of a very faint tint. But, if the latter eye was exposed to a white light during the first part of the experiment, I invariably perceived the same spectrum with this eye as I did with the left one.

This experiment shows that the presence of white light is not necessary for the perception of complementary ocular spectra, and further would appear to indicate that for a sympathetic spectrum to be excited in the eye which has not been exposed to the colour-stimulus, the excitation of some light is necessary.

M. Plateau painted one half of a piece of paper red, the other green; and after alternately directing the eyes to each half, covered them with a handkerchief, and observed a black image, having on each side a complementary coloured image.† He hence inferred that "the combination of accidental colours produces black." Sir D. Brewster very properly objects to this conclusion, "because the eye has been in succession rendered insensible to the two colours which compose white light itself."‡

* Brewster's Optics, 1831, p. 305.

† Annales de Chimie for 1833:

‡ Lond. and Edinb. Phil. Magazine for May, 1839, p. 335.

Elsewhere the same author says, "If we take the two complementary colours, namely, the *red* and the *green* tints forming the *ordinary* and *extraordinary* pencils in the polarized ring, which, by overlapping, form *white light*, then it is manifest that the accidental colour of the overlapping part is *black*, and hence the sum of the action of the *red* and *green* acting separately must also be black."*

Notwithstanding, however, the authority of Sir D. Brewster, the following experiment which I have performed appears to me rather to corroborate Plateau's view. If the two halves of a card painted red and green respectively, be illuminated by a green or red light, they appear black. In the same way, but depending on a different cause, the two halves of the card, if viewed through green or red glass, appear black.

Another set of observations, connected in a degree with the preceding, may be here noticed. Chevreul† distinguishes two chief species of contrast of colours, *simultaneous* and *successive* contrasts. But an examination of these distinctions shows them in my judgment to be more apparent than real, and but the expression of one fundamental fact, viz., that the eye on perceiving any one colour acquires a tendency to see its complementary. Thus, to take an example of Chevreul's simultaneous contrast:—If a strip of red and one of yellow paper be viewed side by side, near the line of contact the red paper inclines to violet, the yellow to green. The rationale of this is at once obvious: the red mingling with the complementary of yellow, i.e., blue, produces the violet tint; whilst the yellow mingling with the complementary of red, i.e., green, produces a light green; and this same law holds good in the juxtaposition of any two colours whatever. By the term successive contrast Chevreul designates the familiar phenomena of complementary ocular spectra, of which a most comprehensive history has been given by Darwin in the *Philosophical Transactions*, vol. lxxvi. p. 33, *et seq.* Du Tour‡ thought that the two eyes cannot perceive each a separate colour at once. He says that if, e.g., a blue disc be presented to one eye and a yellow one to the other, the result is that the mind perceives alternately the one or the other colour, but not the two at once. But I would submit that these two statements do not include the whole facts of the case. I took two tubes, each $10\frac{1}{2}$ inches long, and applying the end of one to each eye, viewed the sky through them. I found that when the

* Op. Cit. for December, 1839, p. 437.

† "The Principles of Harmony and Contrasts of Colours," by M. E. Chevreul.

‡ *Memoires de Mathématique et de Physique* présentés à l'Académie Royale des Sciences, iii. p. 514, iv. p. 499; Paris, 1760-63.

contiguous edges of the tubes at their further ends were some inches apart, two distinct white circles of sky were seen; these circles touched when the edges of the tubes were from $2\frac{1}{4}$ to $2\frac{1}{2}$ inches apart, and, when closer, the two circles appeared as one. If now the further end of one tube was covered with a piece of green glass, the end of the other with a piece of red, as long as the ends of the tubes were kept not closer than $2\frac{1}{4}$ to $2\frac{1}{2}$ inches asunder, the two coloured discs were perceived perfectly distinct from one another; no alternation of either colour to the exclusion of the other, as in Du Tour's experiment, ensued, so long as the tubes were inclined to each other at this or any greater degree of divergence.

Another very interesting series of phenomena depending on the intrinsic sensibility of the eye to the impressions of colours, are those of coloured shadows. The first exact observations on these were made by Count Rumford.* He observed that the two shadows of an object placed in front of a white ground, from a white and a coloured light, were of the two colours complementary to the latter. I have investigated this fact a little more closely. The method adopted has been to throw a white and a coloured (red) circle of light from two magic lanterns on a white screen, before which a slender wooden rod was placed. It is easy to satisfy ourselves that the red shadow is produced by the (otherwise colourless) shadow cast from the interception of the white light being simply illuminated by the other red light. The green shadow is the shadow produced by the interception of the red light, illuminated by the white light. These coloured shadows have, by Rumford and many subsequent observers, been ascribed to the effect of contrast. But this appears an inadequate explanation; for if, with one magic lantern, a half-white and a half-red circle of light be thrown on a screen, a shadow thrown across the two fields is simply dark, without any colour at all. If, again, a red and a white disc of light be thrown from two magic lanterns respectively on a screen, so as partially to overlap, where the overlapping takes place two complementary shadows of any object are seen, but in the other two parts of the field only one colourless dark shadow is seen.

The following facts seem to form the basis of the explanation of coloured shadows:—Firstly, the experiment of Rumford†—that a piece of grey paper placed next to a piece of coloured paper, both on a black ground with the exclusion of extraneous light, appears tinged with the complementary colour. Secondly, I

* Philosophical Papers by Benjamin, Count of Rumford. London, 1802, vol. i. p. 333.

† Op. Cit., p. 336.

found by my own experiments that if, in a dark room, the screen is illuminated with a red circle of light from a magic lantern, the greenness of the shadow and the redness of the ground on which it appears are inversely proportional to one another. By approximating the red light to the screen this becomes redder, whilst the shadow of the rod placed before it becomes less green and darker, till it becomes an ordinary black shadow; that, on the other hand, removing the red light till it leaves the white screen but faintly tinged with red, brings out the green shadow very prominently, and on admission of light into the room, a second faint red shadow comes out.

Meusnier observed "that when the sun shone through a hole a quarter of an inch in diameter on a red curtain, the image of the luminous spot was green." Another observer, Mr. Smith of Fochabers,* states, "If we hold a narrow strip of white paper vertically, about a foot from the eye, and fix both eyes upon an object at some distance beyond it, so as to see it double, then if we allow the light of the sun, or a light from a candle, to act strongly upon the right eye without affecting the left, which may be easily protected from its influence, the left hand strip of paper will be seen of a bright *green* colour, and the right hand strip of a *red* colour."

From all these facts, I think the conclusion arrived at by Sir David Brewster appears highly probable, that "as in acoustics, where every fundamental sound is actually accompanied with its harmonic sound, so in the impressions of light, the sensation of one colour is accompanied by a weaker sensation of its accidental or harmonic colour."† To this might perhaps be added, that there is a tendency in the eye to, as it were, decompose white light into two complementary colours; and further, that the predominant decomposition is into red and green.

Applying this theory to the phenomena of coloured (*e. g.*, red and green) shadows, the red shadow has already been shown to be simply due to the illumination of a colourless shadow by a red light; whilst, on the whole of the rest of the field of the white screen, the red tint cast from the magic lantern is sufficiently powerful to overcome the green tint which the eye would otherwise perceive, excepting at one spot—namely, that which does not receive any red light on account of the interposition of the opaque rod. Here the green (harmonic) colour, having no antagonistic red to overcome it, is rendered sensible to the eye.

* Brewster's Optics, p. 405; London and Edinburgh Philosophical Magazine for October, 1832, vol. i. p. 249.

† Brewster's Optics, p. 309.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

- I.—*Hétérogénie, ou Traité de la Génération Spontanée, basé sur de Nouvelles Expériences.* Par F. A. POUCHET, Correspondant de l'Institut (Académie des Sciences), &c. Paris. 1859.

THE theory of spontaneous generation has a most respectable origin in antiquity. It is probable, indeed, that from the very earliest ages it has had its supporters; but we know for certain, from the works of Anaxagoras, Aristotle, Pliny, Lucretius, and many others, that the matter had in their day attracted great attention at the hands of all to whom the study of natural history was a subject of interest. We do not, of course, mean to assert that the theories of Aristotle, for example, would be easily recognized under the garb of a modern treatise on spontaneous generation, but we must not lose sight of the fact that the ultimate theory is the same—life arising from death, molecules of organic matter grouping themselves together and becoming endowed *spontaneously* with life. It is curious to observe, and it is an observation which cannot escape the most cursory examination of this subject, how universally this theory has always seemed to be inherent in the minds of all, frequently as a refuge from ignorance, often as the result of patient investigation and untiring labour in the cause of science; but it is, on the other hand, no less interesting to note how the opinions of Aristotle, which were for some centuries universally adopted, are now held in so far only as his main conclusion is concerned, viz., the existence of spontaneous generation, whereas the reasons of belief are entirely changed. Aristotle believed that mice could become impregnated by licking salt, that eels took their origin spontaneously from mud, and that serpents sprung from the earth. But it is not on such ground as this, as may well be imagined, that we are asked in these days to believe in such a doctrine. The domain of natural history afforded many phenomena, to reconcile which with those laws which are now generally admitted to regulate the perpetuation of animal and vegetable life, baffled the skill of successive generations of illustrious observers, and drove them, still hesitating, into a belief which they received with trembling superstition. It were idle here to enumerate, or even to give examples of the extraordinary theories which were propounded on this subject in times up to the middle of the seventeenth century, when learned men soberly announced that

to produce a generation of serpents you had only to reduce one of those reptiles to powder and sow them in the earth, when you might confidently look in due time for an abundant harvest. This, if true, would have sadly bothered St. Patrick in his celebrated crusade against the ophidians, but does not demand any further notice at our hands.

There is one fact connected with the history of this subject which may well serve to point a moral applicable to the study of more than this solitary department of natural history, and which we may advantageously bear in mind in considering what constitutes proof, not only in works of a former epoch, but in our own day. This is to be found in the highly ingenious manner in which some of the supporters of this doctrine of spontaneous generation manufactured evidence for the purpose of upholding their own views at the expense of truth—a course of procedure not, we fear, altogether without parallel in these days of exact science. We shall give an example of this:—

The learned Jesuit who started the theory about the serpents above alluded to,* was one of the most enthusiastic supporters of spontaneous generation of the age in which he lived; albeit, the upholders of the doctrine in the present day are rather ashamed of their associate. There appears, indeed, to have been no limit to his credulity. Fragments of the stems of various vegetables placed in water, became, under his patient examination, transformed into certain animals; and, in order to carry conviction with this statement, he appended to his work elaborate plates, which clearly showed the animals in question in their various stages of transformation from the vegetable fragments. To this example we might add many more, such as may be found, for example, in the works of Aldrovande and S. Munster, where it is clearly explained and delineated, how from certain vegetables, certain animals spring, but any multiplication of such fables would serve no good purpose in reference to the object of our review.

Redi, the celebrated Florentine naturalist, was probably the first who boldly faced the arguments of Kircher and his followers, plucked the mask from their sophistry, and exposed their credulity, if not dishonesty; and in so doing, he established a new epoch, when every theory on this subject was subjected to the most searching tests, the results of his brilliant investigation being to scatter to the winds the above and many other time-worn fallacies. By simply protecting some meat by means of a very fine gauze, he demonstrated to a gaping public that the spontaneous generation of maggots was due, not to the putrifying

* Kircher. *Mundus subterraneus*. Amsterdam. 1778.

meat, but to the eggs which the flies deposited there, only because instinct guided them to a place where all the conditions requisite for bringing the ova to maturity were present. Swammerdam, Vallisneri, Réaumur, and a host of others following in the same direction as Redi, demolished what was left of the theory, to such an extent that only at long intervals and in feeble accents were voices raised in its favour. But, while naturalists added to their knowledge, further difficulties from time to time arose with reference to this question of generation. The great book of nature was not to be read without much study, and the task was not to be accomplished without many blunders in syntax, and here, again, came into play that inborn yearning after the marvellous, to which we have already alluded as inherent to a greater or less degree in us all, and which seems to turn the human mind towards this theory of spontaneous generation as a resting-place from mere abstract speculation. We shall not here follow the progress and vicissitudes of this theory, nor show how its supporters were forced to abandon the natural history of the insect world as proof of their doctrine, but will pass on at once to more recent times, when the ground was narrowed to the intestinal worms, and the animal and vegetable life which is developed in *infusions*.

Our reason for stopping short at this period, and not at once proceeding to a consideration of the subject as it stands at the present day, is in order to show how strong the reasoning in favour of spontaneous generation in the case of the entozoa was, and how all but convincing the arguments were. This consideration we may find most useful in weighing the evidence which has lately been presented to us in the recent work on spontaneous generation now before us. Allen Thomson, writing some twenty-five years ago,* after expressing a conviction that the theory of spontaneous generation was the one most in accordance with ascertained facts, states, on comparing the infusoria with entozoa, that the latter "afford still more convincing proofs of spontaneous generation" than the former,† and we give the following extract from his masterly essay on generation, to show those of our readers who are conversant with the present state of knowledge on the subject, how difficult it must be in such matters to distinguish truth from apparent truth. If one of the first British physiologists of the age, and perhaps, as regards generation, our best authority on the subject, has been led so far into error as the researches of Küchenmeister, Von Siebold, and Van Beneden

* Todd's Cyclopædia of Anatomy and Physiology, Art. *Generation*.

† It is interesting to note that Dujardin makes the same observation in his *Histoire Naturelle des Helminthes*. Paris. 1844.

would seem to indicate, how careful ought we to be in coming to any conclusion on facts as revealed to us, while we lose sight of the probability of other facts being hid from us, which, taken along with those already ascertained, could not fail to modify, if not to reverse our opinion. We shall not speculate here as to the possibility of the learned professor being equally mistaken as regards the origin of the infusoria, but will only give his own words as regards the entozoa, leaving all comment to our readers:—

“If entozoa are not admitted to be the product of spontaneous generation, in order to account for their origin, it becomes necessary to suppose either that these creatures themselves or their ova pass directly from one animal to another, or that they are introduced through the medium of air and water. Upon the first supposition, carnivorous animals ought to be affected with entozoa, at least in greatest quantity, if not in some instances exclusively; and the entozoa infesting any particular animal ought to be of the same kind as those which exist in the animal serving it for food. But such is by no means the case. Herbivorous as well as carnivorous animals have entozoa, and in no less quantity; and each animal is the abode of its own peculiar kind. The same entozoa infest the same animals in all localities and climates; thus all the human entozoa, with the exception of the *Dracunculus* or Guinea worm, which is an external parasite rather than a true entozoon, are the same in all races of men. Neither do we recognize any similarity between the entozoa infesting animals of a particular district and allied tribes of animals living in the neighbouring waters.

“In adopting the second supposition that the eggs or germs of entozoa may gain the bodies of animals by circuitous routes, we are met by many difficulties in addition to those already stated in reference to a similar explanation of the origin of infusoria. Many entozoa reside only in particular organs of the body, and in the very interior of these organs, as the human *Cysticercus cellulosus* in the choroid plexus of the brain, in the substance of the brain itself, in the chambers of the eye, &c., so that it is necessary to suppose the ova of entozoa to have been introduced into the circulation, carried through the smallest blood-vessels, and deposited in the places in which they are developed. Animals living in the same situations, and feeding on the same substances, have different kinds of entozoa. The ova of some of the entozoa—as, for example, those of the common round worm (*Ascaris lumbricoides*)—are so large that they could not pass through the largest even of the capillary bloodvessels: the ova are so heavy that they could not be transmitted through the atmosphere; and the supposition of the passage of the ova from parent to offspring is opposed by the mechanical difficulty of the transmission, as well as by the facts that parent and child are not always affected with the same kinds of worms, and that though the complaint of worms may be said to run in families, yet many escape, and one or more generations in the hereditary succession are frequently exempt from it. Entozoa have been observed in the fetus of animals, and supposing them to be introduced from without, it would be necessary to hold that the entozoa themselves or their ova have passed directly from the mother to the child in the uterus, or to have traversed a route through which the globules of the blood are not transmitted.

“Some of the entozoa, we may further remark, when once formed, are viviparous, or bear their young alive; and with regard to these kinds it would be necessary to suppose that they may arise by invisible ova or germs as well as propagate in the viviparous mode.

“These facts appear to us to speak strongly in favour of the occasional occurrence of spontaneous generation—‘a doctrine which, had it not been applied in

many instances where it was manifestly untrue, would have met with less ridicule and a more just appreciation than it has usually obtained.' The epithet 'spontaneous,' which we have retained as the most common, is equally inappropriate as applied to this or to any other of the processes of nature; and the analogy of by far the greater number of plants and animals militates against the probability of the hypothesis; but it must at the same time be held in mind that the organized bodies in which spontaneous production has been said to occur, differ widely in their general structure and functions from those which are reproduced by means of ova; and we are scarcely entitled to reject the hypothesis of their spontaneous generation merely on the ground that, in this respect, they do not agree with the rest of the animal kingdom."

That there are still many points connected with the origin of parasites which are utterly beyond our comprehension, it would but weaken our argument to deny, and we shall, therefore, pass over this without further notice, and proceed at once to a consideration of the question, whether it has been proved, either in the work before us, or elsewhere, that in regard to the myriads of living organisms which appear in infusions under certain conditions, we must, once and for all, discard the hitherto known laws of generation as impossible, and admit this theory to be a proved fact, involving as this does, the admission of a creative power specially exercised in their favour.

Such, in fact, is very much the point at issue. Are we to believe it or not? But, before proceeding to a discussion of this, and to a consideration of the work which calls forth the discussion, we would only remark at the outset that we decline entering upon the metaphysico-theological questions which have been raised on this subject, and which are alluded to at considerable length by M. Pouchet. Even were we to derive any advantage in a controversial point of view by taking up such weapons, we would decline to do so, simply because we consider it dangerous ground. Science has much yet to learn, and there is certainly much in nature that the highest genius and the mightiest intellect will always fail to reduce to natural laws; but who can accurately state where human penetration must cease, and veil its eyes before the inscrutable, or lay down, as in a chart, the "ultima Thule" of science. We freely concede to M. Pouchet whatever he can make of this line of argument, and we think that he has some right to complain of some of the views which he has unnecessarily taken such pains to refute.

M. Pouchet does not now make his *début* as an author, but has already, in 1847, published a work entitled "*Théorie Positive de l'Ovulation Spontanée*," which had previously been submitted to the Académie des Sciences, and had secured such a favourable report from the commission appointed to examine it, as to obtain for its author the great physiological prize of the French Institute. We mention this in order to obtain for M. Pouchet that

fair hearing which his talent and industry undoubtedly entitle him to.

In a work of nearly 700 pages 8vo, to which is prefixed a very complete bibliography of the subject, and which is illustrated by a few plates, M. Pouchet treats of spontaneous generation, and undertakes to prove, by experimental demonstration, what has been hitherto, for the most part, upheld by theory alone. He frankly admits, as indeed he well may, that certain supporters of his cherished theory have pushed their pretensions beyond the bounds of possibility, sometimes even to absurdity. In this category he names Fray, who, he says, "fell into that excess in pretending to have seen slugs and earth-worms take their origin in the midst of fermenting organic substances;" and he might well have added the name of Mr. Crosse, who did not hesitate to give to the world the still more preposterous assertion that he, Mr. Crosse, had manufactured an *acarus* by means of electricity. Adopting to a great extent the arguments and views of Needham, who published in Paris in 1768, his "*Nouvelles Recherches sur les Découvertes Microscopiques*," M. Pouchet limits his observations almost entirely to the infusoria, which class constitutes the citadel to which the supporters of the theory have now retreated. And we are forced to admit that this citadel has hitherto been found to be impregnable; that is to say, that neither by arguments nor by experimental demonstration have the opposite party been able *conclusively* to establish the erroneous nature of the theory.

Were we to view the matter in this light, however, we would place the opponents of the theory in a most unfavourable position, and abandon to its supporters an important vantage ground. But this is far from our intention, believing as we do, that, while the matter is far from being either proved or disproved, all reasoning from analogy is against the theory, and that, therefore, the *onus probandi* lies with M. Pouchet and his friends. Not so, however, thinks M. Pouchet. He knows full well that the whole historical bearing of the subject, and the analogical reasoning drawn therefrom, is against him and his theory, and, therefore, he objects to our borrowing any arguments from times gone by. "*Laissons*," he says, "*à chaque siècle la responsabilité de ses doctrines et de ses erreurs, et n'entravons point la marche du nôtre en accumulant les fautes des autres époques; la gloire de sciences modernes n'a plus à compter avec les veilleries des temps passés.*" To this request we cannot agree, as it would suit our purposes as critical judges as ill, as it suits M. Pouchet well in his capacity of a special pleader. The whole history of the subject from the earliest ages down to the present day, is, in so far as it may be admitted as evidence, decidedly against the doctrine which M. Pouchet

upholds. What, in fact, does its history consist of, but an enumeration of the various apparent paradoxes in natural history, which were only with difficulty, and after generations of close and accurate investigation, reconciled with laws already revealed? Step by step it was forced back, and, instead of advancing *pari passu* with the march of science, it was driven to take refuge among the infusoria, many of which are, when fully formed, barely visible through the most powerful lens.

Our readers are well aware of the conditions under which certain animalcules and cryptogamic vegetable organisms make their appearance in infusions. There must be present organic matter capable of undergoing putrefaction, and moisture, and atmospheric air are also probably indispensable, although some experiments would seem to indicate the contrary. It is also more than probable that light, heat, and electricity, play an important part in developing the special condition in which life appears in the fluid. The question to be answered, then, is—whence comes this life which peoples these infusions with its countless thousands? And, in order to answer this, we must take one of two hypotheses; and as our object in this is truth, and truth alone, we must calmly study the whole subject, and such proofs as the literature of the subject affords, observing, however, great care in our judgment of the latter, until we consider ourselves justified in adopting one view or the other. We must, in short, either take the theory of spontaneous generation as our belief, or prefer to suppose that all nature is teeming with the germs of these microzoa. Now, if we consider that a single drop of water may contain, according to Professor Owen, *five hundred millions* of the smallest varieties of the infusoria, we will at once see that to hope anything here from the microscope would be rather too much. Strange it is, that this invaluable and now indispensable instrument should, while exposing the fallacy of spontaneous generation in one direction, be bringing to light in another a mighty fauna to afford it a more lasting refuge. Let us now see in how far M. Pouchet has succeeded in proving his theory to be the true one. He appeals to critics to read before they judge. This condition, at least, has been scrupulously observed in our case, and if we arrive at conclusions different from him, or find that he has failed in the proof he has undertaken, we can assure him it is not without a careful perusal of his work, and a reperusal of many doubtful passages.

M. Pouchet, in order to establish his case, proceeds on the supposition above stated, that if we do not attribute their appearance to spontaneous generation, there must be germs somewhere from which they spring. He thereupon goes on to show, taking up *seriatim* the organic substance, the water, and the air, that in

none of these media can germs exist. We confess that many of his detailed experiments staggered us not a little in our belief, and it required a more critical analysis of the work to show that, startling as were the facts put forth, they yet lacked something to carry conviction with them. Besides, there are in the book itself certain discrepancies which serve to indicate clearly that M. Pouchet is too anxious an advocate of his cherished theory, to make his book more than an *ex parte* statement, executed with great ability no doubt, but, we fear, without that honesty of purpose and strict regard for accuracy which invariably characterize the works of the true searcher for truth; whereas, he who looks only for proof of preconceived theory cannot be accurate. Human nature is too much for him.

Before taking up the various points which constitute the substance of this work, we would remark, with reference to the authorities which M. Pouchet ranks under the banner of spontaneous generation, that he has in many cases done so without due authority, citing from the works of individual authors sentences which, taken without the context, would seem, at first sight, to warrant him in so doing; but a reference to the works in question will cause us to receive the statements made further on by M. Pouchet with more hesitation than we would otherwise have done. In a most able essay on the subject of spontaneous generation which has lately come under our notice,* and in which M. Pouchet's work is treated of among other things, this is most forcibly indicated. The essayist points, as an example of what we have stated, to the manner in which the names of Valentin and Johannes Müller are borrowed to prop up his theory:—

"M. Pouchet," he says, "had something to gain by citing these names. But what will the reader think of M. Pouchet's trustworthiness on learning the grounds on which these names are cited? Valentin instead of being, as M. Pouchet asserts, a believer in spontaneous generation, is an unequivocal opponent of it. In the very work referred to by M. Pouchet, it is said: 'On the whole, the hypothesis of a spontaneous generation of plants or animals can only be regarded as a *kind of superstition*, which is constantly receding before the advance of the natural sciences.' Nor is Müller, though less decided than Valentin, fairly claimed as upholding the hypothesis. He decides *against* all the experiments which had been thought to prove it, and doubts whether it is possible to prove it by direct experiment. Yet M. Pouchet does not hesitate to say that 'vanquished by evidence, this great German physiologist admits spontaneous generation in its strictest sense—almost as Aristotle admitted it.' Now the simple truth is, as any one may see on turning to Müller's pages, that it was only the unexplained facts of parasitic life which made him hesitate; on all other grounds he is as decidedly against the hypothesis as a cautious man could be. Yet M. Pouchet more than once quotes the opening paragraph in which Müller says, 'we must inquire whether, under certain conditions, simple organic beings may not be generated from dead organic matter;' and this

* Blackwood's Magazine. February, 1861. Art. *Spontaneous Generation*.
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inquiry, which Müller answers in the *negative*, as far as he answers it at all, M. Pouchet chooses to quote as showing that Müller was vanquished by evidence!

"The adherence or antagonism of Valentin and Müller can be of little importance to one who professes to bring abundant experimental proof; but at any rate we require that, if they are cited at all, they shall be cited accurately; and when we see a writer citing superfluous authorities with culpable inaccuracy—making one of them say the direct contrary of what he says—we are obliged to receive with considerable scepticism any statement this writer makes on more complex facts. Every one knows how difficult it is to make an accurate statement of all the conditions and phenomena of a complex experiment. The sincerest investigator may overlook, or forget to record, details which, if seen and recorded, would perhaps greatly alter our appreciation of his inference. But to report truly the expressed opinion of another, when that expression is unambiguous, is surely not beyond the capacity of any sincere writer. When, therefore, we find M. Pouchet so carelessly inaccurate where accuracy was easy, as in the case of Valentin, it is inevitable that we should suspect him of oversight and inaccuracy, where accuracy is of excessive difficulty—as in delicate experiments."

The names of Owen, Allen Thomson, and indeed of all of any eminence who inclined to think this theory possible, he glibly quotes, not as if they had admitted it as a theory alone, but as if they too were convinced by proof. Even from the works of Harvey he does not fail to bring up an ambiguous sentence, as if he could expect us to believe that he was an advocate of spontaneous generation—Harvey, whose aphorism "*omne vivum ex ovo*" is as celebrated in the scientific history of his country, as the memory of his genius is cherished. Nor does he hesitate to quote Redi himself in the same way, who says that "he is *inclined to believe* that, since the creation, the earth has of itself produced no organized being," and adds, "that it appears to him that *in all probability* the different varieties perpetuate themselves by means of seeds." The italics are M. Pouchet's, who chooses to assume that, because this illustrious naturalist declines, with characteristic caution, which M. Pouchet himself would do well to imitate, to express his own convictions too boldly, he may therefore be ranked among his supporters. From all this, our readers will, we think, agree with us in recognizing the necessity of examining our author's experimental proof with the greatest strictness.

M. Pouchet undertakes to prove:—

1. That the germs or spores do not exist in the organic matter itself.
2. That they do not exist in the water.
3. That they do not exist in the air.

"If it be admitted," says he, "that in our experiments, generation cannot take place but by the aid of three factors, *and that it is one of them only that conceals the germs of the proto-organisms*, it is evident that if one of these bodies be taken by itself, without taking the two others into account at all, and if it be

successively proved that none of them contain germs, we must admit when the fact has been strictly established as regards each, when isolated, that none of these bodies can possibly serve as a resting-place for the eggs or invisible seeds of the various organisms which we see appearing before our eyes."

1. In examining with M. Pouchet the first of the propositions set down above, we would beg our readers to notice, in the first place the words which we have placed in italics in the sentence above quoted, as we shall have occasion to refer to them again. In order to prove that the organic substance cannot contain germs, he subjected it to a very high temperature (250° C.), and yet he succeeded in developing infusoria with the charred matter. This proves, we admit (for we cannot well suppose germs to be incombustible), that any germs from which the organisms in question sprung, could not have been in the organic matter.

2. As regards the water, M. Pouchet expresses a conviction that it is one of the elements which is indispensable to the development of proto-organisms. We find, in this part of the work, some experiments of great interest, which seem to indicate that the depth at which the organic body is placed beneath the surface of the water exercises a most extraordinary influence, not only on the number of microzoa developed, but also on the varieties developed, which are quite distinct, according to the distance intervening between the organic matter and the atmosphere—the development being much retarded if a high column of water be placed on the organic matter. M. Pouchet found that when operating with distilled water, he again was successful in obtaining the same result as in the former case, thereby proving that the appearances in this second series of experiments could not be due to the existence of germs in the water employed.

3. In proceeding to disprove the existence of germs in the air, M. Pouchet seems to have been quite aware that he was approaching a subject, to dispose of which as he wished, would give him no little trouble. Experiments had here to be met by counter experiments; and there were great difficulties to be apprehended in the management of apparatus, to the complete exclusion of atmospheric air. He did not hesitate, however, to attempt the task, and we must indeed admit that, in the execution of this, he has displayed great ingenuity and great perseverance. He first sets himself to repeat a well-known experiment, which was performed by Schultze,* and which was considered, at the time it was published, as conclusive, or nearly so, of the presence of germs in the air. This experiment was as follows:—In a flask were placed certain animal substances, and the vessel was then

filled with distilled water. After boiling, in order to destroy any germs which might be in suspension, the flask was closed, the cork being perforated by two tubes, which communicated with two of the Liebig's bulbs used in organic analysis. One of these bulbs was filled with sulphuric acid, the other with potash. For two months Schultze daily sucked the air from the apparatus, so that the air which replaced that withdrawn should pass through the sulphuric acid, but at the end of this time not a single organism was to be seen; whereas, in an open flask standing beside the former, monads, vibriones, and polygastria were seen in abundance. Three days after the opening of the first flask, infusoria appeared, whereas no trace of organic life had been observed until the cork was withdrawn. M. Pouchet makes a great deal of these somewhat inaccurate experiments of Schultze. He repeats it with additional precautions, but with results diametrically opposed, as the following will show:—

"I repeated in the following manner the experiment of M. Schultze:—A flask of the capacity of a litre was half filled with water, and five grammes of hay were placed in it. The cork of this flask was traversed by two tubes, bent at right angles at a short distance from the mouth of the flask. One of these, the afferent tube, did not go deeper than the neck of the vessel, while the other, the efferent tube, was plunged beneath the surface of the liquid, in order to remove the heavy gases which might stagnate on its surface. Each of these tubes was articulated, by means of a cylinder of caoutchouc, with one of Liebig's bulbs, which was filled with concentrated sulphuric acid. In order to secure still greater precision, and to render the introduction of the air slower and more regular, and, consequently, to wash it better in the acid, we employed an aspirating vase, which, when the stop-cock was slightly opened, drew out the air globule by globule. This vase received a tube which was articulated with the efferent bulb. The cork of the flask having been luted with copal and vermilion varnish, and the extremities of the caoutchouc cylinder having received the same luting, a spirit lamp was placed beneath the apparatus, and the water in the flask was soon raised to ebullition. This was kept up for an hour, during which the vapour escaped in great abundance by the tubes. The lamp was then extinguished, and the flask slowly cooled, a little air being from time to time withdrawn by the bulbs. The evening of the first day, when the apparatus was perfectly cold, a litre of air was passed through the flask as gradually as possible by means of the aspirating vase, which had been luted to the rest of the apparatus. Every day thereafter the same operation was repeated with the same precautions, either for the removal of the gas which was produced on the surface of the liquid, or in order to furnish respirable air to the animalcules which might appear in the decoction. Near to this a criterion was placed, in the form of a flask, exactly the same as that used in the experiment, having received the same quantities of water and hay, and in which these had undergone the same ebullition; the mouth only was left unclosed.

"The progress of this experiment was somewhat slow, on account of the season at which we operated, and the low temperature which prevailed, and which in mean did not rise above 14° Cent. The fluid became slightly coloured, and remained perfectly diaphanous and of a reddish tint until the twentieth day, when it became a little nebulous and muddy, and a slight deposit took place at the bottom. On the twenty-fourth day a little greenish speck was

observed to form on the surface, which appeared to be formed of a species of *Penicillium*. The following day another was seen of similar dimensions.

"Finally, on the twenty-sixth day the apparatus was opened, and the following is an enumeration of what we found in the contained fluid:—The two specks were actually formed of a *Penicillium*, very similar to the *Penicillium glaucum*, Link, but more branched, and with very small articulations. The water was peopled on its surface with an immense quantity of the *Spirillum undula*, and of the *Spirillum volutans*. Many other vibriones were also found of very small size and very agile. The pellicle on the surface was arachnoid and formed of large dead vibriones completely entangled. Here and there were seen certain monads difficult to determine. An abundance of the *Bacterium articulatum* was also met with in this decoction, of a length which varied from 0.0076 to 0.0110 of a millimetre. There was seen, besides, in certain places, some animalcules which I believe to be undetermined. These last were cylindrical, truncated at the extremities, and inclosing in their interior from three to five large granulations. Their mean length was 0.0200 of a millimetre. There existed here and there a few seeds of *Penicillia*, easy of recognition.

"In the criterion, vibriones appeared on the third day, and died on the fourth. On the sixth, the *Penicillium glaucum* appeared on the surface; and on the fourteenth, the specks of *Penicillium* were much enlarged, and the liquid contained absolutely nothing but a large quantity of the *Monas elongata*. On the twenty-sixth day, that on which the apparatus was opened, its fauna was found to be not more advanced than that of the latter; a few of the *Monas termo*, but no *Polygastria*, as described by M. Schultze."

Our object in quoting the above experiment as given by M. Pouchet, at such length, has been to afford our readers a fair average specimen of the manner in which his experiments have been conducted, and of the apparent care and minute accuracy with which all his observations have been made. But his experiments go much further than is indicated by that which precedes, as we shall be able to show without giving the experiments at length:—

1. In order to destroy the germs in the air supplied to the apparatus, he passed it, before entering the flask, through a red-hot tube. Various varieties of vibriones nevertheless appeared.

2. Ten grammes of hay, which had been carbonized in a stove, were put into a flask, which was then filled with cold filtered water. The air supplied to this apparatus was then passed through a red-hot tube. In two days, the apparatus being opened, a microscopic examination revealed an abundance of vibriones, of the *Monas crepusculum*, and of the *Bacterium articulatum*.

3. Not content with a long series of experiments similar to those which we have selected, M. Pouchet now proceeds to narrate an experiment where artificial air is employed, instead of the atmospheric as before. Having filled a large flask of the capacity of five litres, and provided with an accurately ground stopper, with boiling water, he stopped it and inverted it over a mercurial bath. After the water had cooled, a mixture of nitrogen

and oxygen, in proper proportions, was introduced, so as to fill three quarters of the vessel. Some hay which had been exposed in a stove to a temperature of 100° Cent. (212° Fabr.) was then introduced, under the strictest precaution against the admission of the smallest quantity of atmospheric air. After this the stopper was introduced beneath the mercury, and the flask removed, a luting being carefully applied to avert any possibility of error. The water commenced to appear nebulous about the eighth day, when several greenish specks were observed floating on the surface of the liquid. On the twelfth day, a spherical globule was seen near the bottom of the mixture, and was believed to consist of a mass of *Aspergillus*; and on the eighteenth, a mass of *Penicillium* in fruit was observed. In a month the apparatus was opened, when, in addition to the organisms above named, numerous animalcules were found of the following species—*Proteus diffuens*, *Trachelius trichophorus*, and *globifer*; *Monas elongata*, and an immense number of vibriones as usual.

4. An experiment precisely analogous to this was performed, pure oxygen gas being substituted for the artificial air, and in ten days the apparatus having been opened, its contents were examined with care. In this case no animalcule was discovered, but vegetable life was abundant in the form of an unknown *Aspergillus*, which was subsequently designated the *Aspergillus Pouchetii* by M. Montagne. "J'ai respecté sa décision," says M. Pouchet.

5. By a series of more than a hundred experiments, M. Pouchet has convinced himself that the number of animalcules had no reference to the *quantity* of air to which the fluid was exposed. In one of these, a man for three days was kept injecting air by means of a pump into a recently-prepared infusion, a criterion being allowed to stand untouched in its neighbourhood. Ten days afterwards, on comparing the two, absolutely the same animated population was found in each, and in the same number. This experiment he repeated on a colossal scale, "in order," as he says, "to paralyse all objection." In a series of vessels, divers macerations were placed in the ventilating shaft of a large furnace. During two hours a current of air was passed over them, which was calculated at six millions of litres in all. After this the vessels were removed to the laboratory, and placed alongside their "criteria." A most minute examination of the various infusions employed, showed a result perfectly similar to that in the simpler experiment above noted.

6. In the same atmosphere, and with the same water, M. Pouchet made infusions of various organic substances—aconite, flax, meat, bones, China aster, and hay. After a certain time a

different fauna and flora was found in each infusion. Why, asks M. Pouchet, if the air is the disseminator of these germs of which we talk, does it not produce in each infusion similar results?

7. A maceration, the surface of which is abundantly covered with the *Penicillium glaucum* in fruit, is placed close beside three other macerations which are free from all cryptogamic vegetation. In none of these three did the *Penicillium glaucum* appear; and moreover, the most careful examination of their surface failed to discover the slightest trace of the spores of this plant.

The above experiments are but the outlines of a few of those which appeared to us to be the most striking among the large number which M. Pouchet has so elaborately detailed; but we think that even the meagre sketches above given are quite sufficient, at first sight, to shake the belief of the boldest opponent of the theory. But we hope to convince our readers that M. Pouchet has stopped far short of proving his case.

In commencing our analysis of what M. Pouchet undertakes to prove, we quoted some preliminary remarks in which he proceeds, in a rather elaborate syllogism, to show how, if we admit his three preliminary propositions regarding the organic substance, the air and the water—with one trifling addition, of which he nowhere offers proof—we must admit his theory as true. Perhaps M. Pouchet will allow us critically to examine his premises before we subscribe to his conclusion. Granted, for the moment, that he has proved that germs do not exist in the organic substance *employed in certain experiments*; that, in another series, he has proved that the water used cannot contain them; and thirdly, that life appears although there cannot be germs in the air or other gas employed. Granted even that in two of these he may have proved the absence of germs, as in the experiments in which he operated with artificial air on carbonized organic matter; but we must add that we have failed to recognize even an attempt on his part to prove that in the three elements, *as employed in any one experiment*, germs cannot be present. And yet with an amazing assurance he does not hesitate to substitute for this *experimentum crucis* a mere theory, as will be seen on a reference to the quotation, and especially to the passage which we have italicized. We confess ourselves utterly unable to see on what ground M. Pouchet asks us to admit that, if germs exist at all, they must exist in one of the three elements, to the exclusion of the others. Had he proved this, indeed, it would, along with what he has established, have gone far to convince us. That there is a theory which supposes all nature (organic matter, air, and water together) to be teeming with germs of infusorial life, and it may be with germs of organisms so minute as themselves to be invisible to the highest microscopic power, is a fact

which he here appears to ignore. And yet in other portions of his book we find him sneering at this very doctrine of "panspermie," of which we will only say that, as a theory, it appears to us to be superior to his own.

As regards his experiments with reference to the organic matter, we cannot for a moment imagine that any germs could survive a temperature of 250° Cent. (546° Fahr.). But we think it a little suspicious that, in some of the experiments in which he professes to destroy any germs which may be in the organic matter, he does not think proper to raise the temperature above 100° Cent. (212° Fahr.). Observe that he has already proved that all of the organic substance that is necessary for fulfilling the part which it plays in his assumed spontaneous generation, must be found in the residuary carbon after ignition, and the inorganic salts. This is a conclusion which the truth of his own experiment must force upon him; and we repeat that it is, to say the least, a little suspicious that he should, in an experiment (see Exp. 3) where he kills the assumed germs in the air, submit the organic substance to a temperature, which the following experiment from the essay formerly quoted shows to be inadequate to destroying the vitality of germs:—"Not willing," writes the essayist, "to take any material point for granted, we boiled the germs of the *Aspergillus* for ten minutes, and found *that boiling did not destroy them*. We then again boiled for twenty minutes these same germs, with the paste on which they were, and again found them perfectly unaltered. Still further to test this important point, we boiled other germs for one hour and a quarter, yet these were perfectly unaltered."

The experiments to prove that germs did not exist in the water, in so far as distilled water is used, are conclusive; but when he only boils the water, and assumes that all germs must be killed thereby, he lays himself open to criticism, and his conclusions are refuted by experiments such as that which we have just quoted.

His experiments with atmospheric air and other gases is evidently the part of his book to which he has devoted the greatest attention. Some of the experiments are truly wonderful, and most of them are of the deepest interest. His patient observation, and the elaborate record of the various forms of animal and vegetable life which came under his eye, undoubtedly entitle him to a high position in the scientific world, and to the highest praise even of hostile criticism. But, with all the ability with which we gladly admit he treats this part of his subject, we regret to find that straining after effect, and unfairness in treating of the views of others, which we have remarked throughout all his work. The special objections which we have to find with this section of his work, as

elsewhere in it, are derived from the manner in which he assumes certain things as proved which suit his theory. For example, he begins by showing that the conclusions of Schultze are as false, as his experiment was insufficient; and yet he adopts an apparatus almost identically the same, and, in all his future experiments, assumes that germs must necessarily be killed, if the air which contains them be passed through concentrated sulphuric acid, a fact which is extremely doubtful. But, be this as it may, is it not more rational to suppose that, in this experiment at least, the germs are in the water or organic substance, and the oxygen necessary to their development passes freely through the sulphuric acid tubes? Indeed, even were we forced to admit that the boiling has destroyed the germs in the two situations alluded to, we would still prefer to suppose that the germs passed unhurt along with the spherical masses of air in which, while passing through the liquid, they might possibly enough be preserved.

Some of the most remarkable experiments of M. Pouchet are those in which the vessels were perfectly filled with a boiling decoction and hermetically sealed and, nevertheless, cryptogams, such as the yeast plant, appeared. That this should appear without the presence of either atmospheric air or oxygen, is somewhat startling to one's preconceived ideas of life in general. That life can be maintained without these agents, we may admit; but that development can go on and myriads of new beings spring into existence, absolutely without them, is incredible. What, then, does M. Pouchet mean by saying, when he combats the experiments of others, that oxygen is essential to organic development, and then adducing experiments of his own to prove that his own statements are untrue? This glaring inaccuracy is pointed out by the writer in *Blackwood*, who makes no remark, but leaves facts to speak for themselves.

We pass now to the portion of his work in which M. Pouchet traces and describes the development of the microzoa. He here commences by stating that the formation of animalcules in macerations is preceded by an evolution of various gases, the products of the decomposition of the organic substance—a statement which is in all probability strictly correct, but which we have some difficulty in reconciling with some of his previous statements. The next step in the development of the proto-organism, he states to be the formation of that film on the surface of the fluid with which we are all so familiar, and which is composed, for the most part, of dead animalcules, at first of the very lowest classes, and subsequently of orders higher in the scale. This film he names the proligerous pellicle (*pellicule prolifère*), and he ranges these under various classes according to the forms of infusorial life of which they are formed. Proceeding now to

what he calls the *secondary phenomena*, that is, the apparition of the spontaneous ovule in the proligerous pellicle, he broaches a theory which, though not original in itself, is supported by him with some originality. The opening paragraph of this section will show this :—

“In comparing attentively the various phenomena of development which successively manifest themselves on the appearance of *spontaneous ovules* and *ovaric ovules*, I do not see the slightest difference between the two, if it be not that the former are the result of the concentration of organic molecules from the stroma of the ovary; while the latter are produced by that of organic molecules from the proligerous pellicle. And if, in what follows, there be a fundamental difference between the two bodies engendered, it is in favour of that which is spontaneously born, for the ovaric ovum, in order to continue its evolution, generally requires to be fecundated, whereas the spontaneous ovum, raised to higher biological power, goes through all its phases without any new stimulant.”

To say that the above theory was impossible would be to go too far, but we have no hesitation in saying that it is in the highest degree improbable. To follow M. Pouchet in his long history of the method in which the ovule is generated and becomes a perfect animalcule would, we think, be neither interesting nor instructive to our readers, in so far as it bears on the theory above stated. But, had we space here to enlarge upon the subject, there are many of M. Pouchet's observations which to the physiologist are of the deepest interest. The plates which he gives us may be perfectly truthful, and due in no degree to the mental effort which guided, as we have seen, the pencils of Kircher and Aldrovande; but we must confess that we have failed to discover here, among the numerous observations he records, one solitary fact which is not perfectly reconcilable with the existence of germs. We do not think we would draw so much on the credulity of the public as M. Pouchet does, were we to ask them to believe that all these recorded facts were due to pre-existence of germs; we only ask them to believe that where certain animals are so minute as to be barely visible with the highest microscopic power, their germs must be quite invisible, and that those appearances which M. Pouchet so industriously records, are simply the various phases of the development of germs which the present state of optical science, perfect although it appears to be, does not permit us to discover. Of this, however, the following extract will enable our readers to judge :—

“The series of phenomena which are seen successively to appear are as follows:—When the proligerous pellicle has become perfectly homogeneous, and its granulations are uniformly composed of the dead bodies of the *Monas crepusculum* or *Monas termo*, the first indication of creation (*genèse*) that the observer can perceive, consists in small masses of these granules, which join from here and there in this membrane, and at distances nearly equal. These masses are due simply to the grouping or concentration of the granules which

surround the vital centre, as if the latter had drawn them towards it at the expense of those surrounding it at a greater distance. From this it results that this first indication of an ovule, for indeed it actually is one, presents granulations a little denser than the proligerous pellicle itself, while, on the contrary, there is all round the central mass a zone somewhat clearer and very broad. This zone, of which the limits are not accurately circumscribed, forms in the pellicle as many clear circles as there are ovules in the act of formation under the field of the microscope. But all is yet confounded with the proligerous pellicle during the first efforts at organization. These masses of granules constitute the first vitelline granules of the ovules of the microzoa."

Were we to enter into the questions which follow this section of M. Pouchet's work, we would be led from the immediate subject of our review to a consideration of such subjects as the succession of creations, the immutability of beings, and the like, a task which we could not undertake, however superficially, within the limited space which is left for the conclusion of this paper. We shall just give a glance at the proofs which he draws in favour of his theory from the intestinal worms; but here the ground has been so knocked from under his feet that he makes but a feeble stand, dismissing the subject in a few pages.

As was to be expected, however, he manfully combats every inch of ground. This department of natural history was, as we have already shown, the favourite vantage-ground of the theory, until of late years it has been driven very much to take refuge among the infusoria. In these circumstances, M. Pouchet points to some facts connected with the entozoa which have yet to be explained; but treats, we think, rather cavalierly the discoveries of Van Beneden, and others. These discoveries, however, have reconciled with the usually accepted laws of generation, facts as strange and apparently paradoxical as any of those which we have hitherto mentioned. But for the genius of those naturalists who so ably investigated the subject, M. Pouchet would have pointed with equal confidence to the common tape-worm, as additional proof of the doctrine which he holds. As it is, he hints obscurely that the matter requires more investigation, as to the identity of the *cœnurus*, *cysticercus*, and *echinococcus*, with the various forms of the tape-worm. And yet to us there really appears to be nothing wanting here in the way of proof. Von Siebold was the first who announced that the *cysticercus* found in the liver of rats and mice was nothing but the *tænia* of the cat, and both he and Küchenmeister by a series of splendid experiments succeeded, and we think conclusively, in showing that this was the case. Perhaps the following well-known case, as narrated by Van Beneden in his "Memoire sur les Vers Intestinaux," is one of the most striking that we could adduce. This illustrious naturalist gave to a puppy, several parasites taken from the body of a rabbit. These parasites belonged to

the genus *cysticercus*, which he assumed to be the larval form of a tape-worm which is never found in the rabbit, but always in some animal which has eaten the rabbit. With this puppy, another of the same age was kept, to which no parasites had been administered; and, this latter dying in a few weeks, no tape-worm was found on dissection. Three other pups were then obtained, in addition to the one to which parasites had already been given, and one of these Van Beneden fed with parasites, while to the other two none were administered. Could anything be more convincing than the result which follows? Van Beneden brought with him these four dogs to Paris, and there in presence of Valenciennes, Milne Edwards, Quatrefages, and Jules Haime, he made the following statements:—"In two of these four dogs you will find specimens of the *Tænia serrata*, and in two you will find none. Further, judging from former experiments and from the time at which the parasites were given to the two dogs affected with *tænia*, I affirm that in No. 1 you will find the *tænia* in four different stages of development, whereas in No. 2, you will find but three." On dissection, his assertions were found to be strictly correct.

The following extract from Von Siebold's monograph will, we think, leave no room for doubt:—

"Of experiments tried with *Cysticercus pisiformis* upon ten dogs, the following are the results which I repeat from the report I made in my essay in the *Zeitschrift* cited above.* When the encysted cysticercci are devoured, it is the cysts which are first attacked by the gastric juice in the dog's stomach, and then the caudal vesicle is consumed; but not, however, the remaining part of the cysticercus, so that of the whole *Cysticercus pisiformis* nothing more is left than the whitish round body which was inclosed in the caudal vesicle, and which consists of the head and neck of the animal involuted within its body, or, in other words, is the scolex. Even before the caudal vesicle is digested, it frequently shrinks and collapses, its thin contents being discharged, probably by exosmosis, into the thicker fluid of the stomach. Accompanying the latter, the remaining portions of the cysticercci, viz. the tailless bodies with their involuted neck and head, pass into the duodenum through the pylorus. Having reached the duodenum, the heads and necks of the cysticercci are extruded, in order that they may find places of attachment by means of their suckers and hooks, between the villi of the intestine, where they may await the growth and further development of the other parts of their body.

"During the first hours of their sojourn in the small intestine, these outstretched tailless cysticercci (scolices) often present a bloated œdematous appearance; but by degrees the body becomes thinner, probably parting by exosmosis with its superabundance of fluid, and in this manner establishing an equilibrium with the more or less viscous chyle. In all these cysticercci the posterior end is clearly the place where, at an earlier period, the caudal vesicle was attached, as is evinced by a sort of scar, like a notch or incision, from which at first very delicate flakes of membrane depend, the remains of the digested caudal vesicle. Already, after a day or two, the worms begin to

* *Zeitschrift für Wissenschaftliche Zoologie*, Bd. iv. 1853.

exhibit a growth, in which only the body takes part, the neck and head being already fully developed, whilst the worms were still within the peritoneum of the rabbits. Whilst the bodies of the worms, as yet unjointed, and only provided with very close transverse wrinkles, increase in length, the transverse wrinkles, after a day or two, change by degrees into distinctly marked articulations; the joints, which are at first very short, lengthen, and there appears either on the one lateral border or on the other, a kind of papillose elevation, which afterwards becomes the aperture of the sexual organs. In this condition the ingested worms have exactly the appearance of a *tænia*, and only betray their origin by that scar on the terminal joint of their body, of which I have already spoken. After remaining twenty-five days in the dog's intestine they have become *tæniæ*, of from ten to twelve inches long. The growth of these *tæniæ* goes on without intermission, the posterior joints increasing in size, and the reproductive organs in the interior developing more and more, whilst at the hinder limit of the neck fresh joints are continually produced from the transversely wrinkled anterior part of the body. In three months these *tæniæ* attain the length of from twenty to thirty inches and more. In such *tæniæ* the posterior joints seem to have reached their full sexual development. In some of these tape-worms the last joints become cast off, a proof of their having attained their sexual maturity. The eggs contained in the fully formed joints are perfectly developed, and contain an embryo, furnished in the usual manner, with six movable hooks.

"After having thus obtained sexually developed *tæniæ*, that is to say, scolices with sexually matured proglottides, from the *cysticercus pisiformis*, I was enabled to decide to which species of tape-worm these scolices, as the head end, and the proglottides, as joints, belonged; and I recognized in them the *tænia serrata*, which had long been known to infest the intestine of dogs. The form of the head, the number, shape, and arrangement of the hooks encircling the head, the construction of the joints and of the sexual organs within them, the form of the developed eggs, all persuaded me that I had educed the *tænia serrata* out of the *cysticercus pisiformis*."

We have no intention here of asserting that there are not many facts connected with the history of parasites which are difficult of explanation, and the nature of which we are perfectly unable to understand, but we must refuse to accept that as any reason for our accepting the theory of spontaneous generation. We may admit, without fear of misapprehension, that in many cases, with our limited knowledge on the subject, we might be forced to say that this is the only theory by which we can account for so and so. But this is far from an admission of the truth of the theory, although it is upon statements such as this that our author assumes that many illustrious physiologists and naturalists agree with him.

M. Pouchet concludes his volume by a statement of the laws of spontaneous generation, in an aphoristic form. These aphorisms consist simply of a *resumé* of the conclusions to which he has been led by his experiments, and are given at too great length for insertion here.

Rising from an analysis of this work we are glad to find that careful study has not shaken our belief in the generally received doctrines of generation, although a mere superficial

reading of such a book could scarcely fail to sap the faith of the unwary. We are far, however, from treating the doctrine with contempt. That which has shaken the belief of the most vigorous intellects, and carried conviction to the minds of not a few men, whose names will ever be celebrated in the world of science, merits careful attention, and, at the same time, perfect respect at our hands. No one now asks us to believe, as Ovid once did, that it is to the earth that we are to look for the source of all animal reproduction:—

“Cætera diversis tellus animalia formis
Sponte sua peperit.”

But we have no right arrogantly to assume that such a doctrine, as it is now limited, is absolutely impossible, however improbable it may be to our minds. We must not forget that all who have made generation a special study, have experienced the difficulty of the subject, and have hesitated, even after years of labour. “I must avow,” says Gleichen “that in spite of all the activity which I have brought to bear on my observation, my patience, and the time which I have employed, it has been impossible for me, even after several years, to say anything for certain.”

Nor must we overlook the fact that some have been convinced, by the most careful study and observation, that the theory of spontaneous generation is untrue. Perhaps the best example we could give of this is to be found in the work of Ehrenberg, whose name is so well known in connection with the infusoria, and whose years of patient labour resulted in his passing over to the ranks of its opponents, after being for long one of the most ardent supporters of the doctrine of spontaneous generation. We have only now to repeat, what we trust we have set clearly before our readers, that M. Pouchet has entirely failed to prove his case. We have treated him, we think, with even more indulgence than he merits, accepting, as we have done, most of his experiments as executed loyally and in good faith. We have at least abstained from impugning his veracity, although, whenever we had internal evidence to bear us out, we have not hesitated to demonstrate his inconsistencies.

II.—*The Modern Pathology and Treatment of Venereal Disease,*
By PATRICK HERON WATSON, M.D., F.R.C.S. Edinburgh.
1861.

It must have struck all who pay attention to the progress of medical literature, how at intervals of varying length the pathology and treatment of certain diseases obtain special attention and

emerge for a time on the platform of discussion, to resume after such an interval of notoriety the even tenor of their way. The subject of the pamphlet before us has markedly undergone such phases of increased and diminished interest, and is at present at one of its culminations, as is testified by the large contributions which almost daily are made to its already prodigious literature, and the constant discussions which its doctrines undergo at our medical associations. Amidst a vast mass of rubbish, the usual tiresome "rechauffés" to which this, like every other medical subject, is constantly subjected, a few really sterling and good contributions have recently been made to the literature of syphilis. It is well known that on many points of fundamental importance, as for example, the unicity or duality of the syphilitic virus; the diagnosis of primary chancre; the communicability of secondary affections; the possibility of acquired protection by syphilitic inoculation; the comparative inoculability of the varieties of chancre; mediate contagion; and various other minor points—a considerable change has taken place in the opinions held by syphilographers; and in the recent works of Ricord, edited by Fournier, of Bassereau, of Diday, of Robert of Marseilles, and of Mr. Lee of London, we have much original research, and much clear and philosophic deduction.

When any subject, such as that now under consideration, is undergoing discussion and renewed developement, and when many works are being written upon it, it is a most acceptable duty that which has been performed by the author of the pamphlet, the title of which stands at the head of this short article. Written originally as a review of some of the recent works on syphilis, it contains a very clear, we would almost say masterly outline, of the whole subject on which it treats; and contains a complete *résumé* of the views so interestingly put forth in the recently published lectures of M. Ricord. When the occupations of all render an appeal to many large treatises inconvenient, and the engagements of many make such research impossible, such an abridgment as this, which contains in a very comprehensive but short form, and in very interesting diction, all that most desire to know, is a really useful contribution to medical literature. Dr. Watson of course does not pretend to any originality; but what he does profess to do he accomplishes most satisfactorily, in that, lightly sketching the rise and progress of the disease, he has dwelt with sufficient length and great perspicuity on the more recent investigations and on the newest phases of doctrine.

Amidst all the confusion and even superstition which has attended the discussion of the question of syphilis, we really seem to have gained some reliable and trustworthy positions from which the other still obscure points may now be investigated,

and in nothing we believe has a more trustworthy advance been made than in the *treatment* of venereal affections. In our day, when so much care is taken to discriminate the cases in which mercury is to be administered, when it is carefully avoided in the case of all patients of certain constitutional tendencies, and in all forms of venereal disease except the true "secondaries," we may expect to obtain the real benefits of a drug which is so powerful an agent for good when rightly employed, and so destructive when wrongly and too lavishly administered. We now hear little of the mercurial and non-mercurial treatment of syphilis, and the better the disease is understood the less will we be reminded of this famous controversy, as all must acknowledge that in cases fitted for it, and in the stage of the complaint to which it is adapted, and in proper quantity and form, mercury is of infinite value in the treatment of syphilis, while no mercury is required in the majority of chancres which are presented to us, simply because they are non-infecting in their character. According to Ricord two in three chancres are non-infecting; according to Puche four to one are of that character, and of course these may be and *should be* treated without mercury; but for the remaining cases, the true infecting chancres, we may heal them, but we will not *cure* them without a judicious course of mercury. Thus it is that maligned and condemned as this drug has been, it is now assuming its proper place among our therapeutic agents, and no proofs of its pernicious influence on the blood can shake its legitimate use when given with discrimination and care. So too the benefit of Iodide of potassium in the tertiary stage, and of combinations of iodine and mercury in the transition forms of the disease, is now so well established as to be reliable.

It is not, however, our intention in this article to enter on a discussion of the "syphilitic question" at all, but merely to draw the attention of our readers to the admirable *resume* contained in Dr. Watson's pamphlet, where they will find all the points of interest connected therewith alluded to. If we had been disposed to cavil at minor points where so much that is good prevails, we would have blamed the silence, or at least cursory allusion made by Dr. Watson to the many circumstances which may change the aspect and progress of chancres, and which introduce so many elements of confusion into their recognition. These points have been admirably brought out by Mr. Lee, who, although somewhat theoretical, we humbly think, as to the phases of inflammation which belong to the different kinds and stages of the syphilitic sore, has yet done good service to the cause, both by his expositions of the intercurrent changes which affect the appearance and normal progress of the chancre, and the mode in which the disease is transmitted in some of its later

forms. Dr. Watson, too, has not taken up the questions of syphilization and the inoculation of secondary syphilis as they most richly deserve; and although we can discover from some of his passing remarks that these subjects have been considered by him, yet we think his *brochure* would have been much more complete if it had contained as careful a digest of these as the other points which it embraces.

III.—1. *Clinical and Pathological Notes on Pericarditis.* By W. T. GAIRDNER, M.D., F.R.C.P., Edinburgh. Pp. 34. Edin. Sutherland and Knox. 1860.

2. *On Insufficiency of the Aortic Valves in connection with Sudden Death, with Notes Historical and Critical.* By JOHN COCKLE, M.D. Physician to the Free Hospital, &c. Pp. 30. London, Davies. 1861.

Two papers on pericarditis appeared in the *Edinburgh Medical Journal* a short time ago from the pen of Dr. Gairdner, which, with the addition of a chapter on treatment, form the substance of the pamphlet before us. Those of our readers who have seen the papers will agree with us in thinking them worthy of republication. Notwithstanding all that has been written on heart diseases, and written so well, something new may yet be observed in this department of medicine, by those who have eyes to see, and ears to hear.

The cases of pericarditis which came under Dr. Gairdner's care during his connection with the Edinburgh Infirmary, formed the subject of his diligent study; and on comparing the information derived from this source with the information communicated by writers on cardiac disease, he concluded that some aspects of the subject were inadequately dealt with by any author. Hence he came to write the present *brochure*. He has had the good sense to refrain from giving a full and systematic exposition of all we know of that disease—such an exposition as he may feel called upon to give the students attending his lectures on Practice of Medicine. Had he done so, the “busy practitioner” for whose instruction these clinical notes are intended, would have been obliged to read much he had read elsewhere. As it is, we have no hesitation in calling upon practitioners, however busy, to spend an hour or two in reading Gairdner on pericarditis, satisfied that when they have done so they will think the time well spent.

The diagnosis of pericarditis, its frequency and results as indicated by examination after death, and its prognosis and treatment are severally considered.

Under the head *Diagnosis*, attention is directed to one physical sign only, viz. *friction-murmur*. What is its diagnostic value? How far, and in what circumstances, may it be relied on as a trustworthy guide to a knowledge of the disease? These questions are answered by the statement of three propositions, each of which is illustrated and we believe amply proved.

1. Friction-murmur is not necessarily present in pericarditis.
2. Its presence is not necessarily a proof of pericarditis.
3. Its distinction from an endocardial murmur is not always easy, nor to be effected by the ear alone.

With reference to the first proposition, Dr. Gairdner is not prepared to prove, although he thinks it probable, that cases of pericarditis *often* occur where no murmur of any kind is present from first to last. What he affirms is, that for purposes of diagnosis a murmur is not to be counted on. "I am willing to suppose," he says, "that the earliest period of pericardial effusion is not often, perhaps not ever, entirely devoid of friction-murmur. But what it is of practical importance to bring into strong relief is, that in the progress of pericarditis this most characteristic phenomenon may be absent for long periods in some cases, just as it may be present for long periods in others; that its presence and its absence bear no appreciable relation to the intensity of the disease; and that therefore you cannot practically make the diagnosis of pericarditis depend on the presence of friction-sound, nor can you gauge the pressure and extension of the disease by the amount and character of the murmur."

It is well known to auscultators that the effusion of fluid into the pericardium extinguishes a friction-murmur, when it occurs to an extent sufficient to separate the roughened and opposed surfaces. That it does not always do so, however, is proved by a case, the details of which are given, where the murmur was distinctly heard notwithstanding the presence of upwards of two pints of fluid in the pericardium; the persistence of the sound being due to "shaggy prominent masses of tolerably firm lymph of spongy consistence and red colour, coating the surface of the heart." Another case, the converse of the above, is narrated, proving that friction-sound once recognized in acute and general pericarditis may entirely disappear, although the effusion may not exceed six ounces, and although there may be a thick coating of lymph over the surface of the heart. The disappearance of the murmur in the latter case, was caused by the prominences of the lymph becoming rubbed and polished away until made perfectly smooth.

The second proposition—that the presence of friction-sound is not necessarily a proof of pericarditis—looks at first sight untenable. By pericarditis, however, we are to understand acute pericarditis. Permanent friction-murmurs arising from abnormal conditions of the pericardium, the result in most cases of former pericarditis, are frequently observed by the stethoscope, attended by no other morbid symptom of disease. The lymph thrown out in an acute attack of pericarditis is sometimes not entirely absorbed, and when it does not cause adhesion of the two layers of the serous membrane, may become the source of a constant friction-murmur. The friction-murmur heard in these cases cannot be said to indicate the existence of pericarditis; it only shows that the disease was once there. Even when adhesion of the pericardium does take place, Dr. Gairdner thinks a friction-murmur may not on that account entirely cease. We shall quote his own words:—"Another element in the diagnosis of pericarditis which is usually stated much too absolutely, is the cessation of the friction-sound on the occurrence of adhesion. While I fully admit that this is often the case, and perhaps it will hold good as a general rule, that all *well-marked* friction-sound ceases with the formation of adhesions, I believe it very far from true that adhesion of the pericardium necessarily leads to the absolute suppression of murmur. Loose adhesions indeed I have, oftener than not, found to be associated with a degree of murmur—a murmur very different, it is true, from the friction of recent acute pericarditis in its most characteristic stage, but quite sufficiently resembling friction-murmurs in general, to be readily suggestive to some persons of a recent attack of pericarditis. And that such murmurs may persist for a long time I have equally good evidence. I have watched them in fact, in cases of old pericarditis, for months together, and lost them only on losing sight of the patient."

In support of the view that lax adhesions are sufficient to cause friction-sound, a case is reported in which, during the last three months of life, a loud and rough friction-sound was heard with the first sound of the heart, and a short and indistinct murmur with the second. After death the pericardium was found to be universally adherent over the heart. The adhesions consisted of a thin layer of cellular tissue, pretty readily broken down, but apparently of some standing.

Another source of friction-murmur noticed, are the milk-patches or white lymph-patches. Opinions differ as to whether these can ever cause anything like a friction-sound. Dr. Gairdner thinks they can; and founds his opinion on the fact that in some post-mortem examinations only white patches were discovered to account for friction-murmur heard before death.

The third proposition respecting friction-sound—viz. the diffi-

culty of distinguishing it from an endocardial murmur—must be acknowledged by all who have had much practical experience in auscultation. Though the one is called friction-murmur and the other blowing-murmur, their acoustic characters are often very much alike; and, if the ear alone be trusted to for the distinction, the most experienced will occasionally make mistakes. The one is also said to be more superficial, and the other less so; but as that is a matter of degree merely, it does not always afford satisfaction. The distinction is only to be made in many cases by attention to the other symptoms associated with the murmur. Dr. Gairdner remarks on this point:—"It is impossible in the face of the recorded experience of Dr. John Taylor and others, to deny the serious fallacies attaching to a mere aural diagnosis of pericarditis, as founded on the recognition of friction-murmurs by their acoustic characters. That these murmurs may be actually so recognized in some cases is indeed true; but in very many instances this is not so; and I have very little doubt that, in even the majority of cases where friction-sound is recognized, it is known to be such by the circumstances in which it occurs, rather than by the mere sound itself. In other words, the recognition of the friction-sound, usually placed as the first step in the diagnosis of pericarditis, is often in fact the last; we recognize the sound as friction because it accompanies the other elements of a diagnosis of pericardial exudation, instead of presuming pericardial exudation to exist simply because we hear a friction-murmur."

Two cases are reported illustrative of the danger of confounding pericardial and endocardial murmurs, when the character of the murmur is alone attended to. In one of these cases Dr. Gairdner himself made the mistake; in the other "a number of educated ears" fell into error, after examining the case repeatedly during a whole month. To avoid mistakes Dr. Gairdner now adopts an excellent plan:—

"For myself I can say that I hardly ever arrive at the conclusion of determining a friction-murmur to be such, without having first applied to it, by a sort of exhaustive analysis, the testing characters of every other well-known cardiac murmur. And by exercising this degree of caution, I feel assured that I have been saved from important errors."

The second part of the pamphlet is devoted to the consideration of the frequency of pericarditis and its results, as indicated by examination after death. The pathological register of the Edinburgh Infirmary is the source of his information on this point. The experience of Louis, Taylor, King, and Chambers, are also referred to. The sources of fallacy in determining a matter of this kind are, of course, very numerous, even where

great caution is observed. The result of his statistical inquiries are as follows :—

1. That general and severe pericarditis—i.e., pericarditis attended with copious deposit of fibrin on every part of the membrane—commonly ends in adhesion.

2. That local exudation from mild pericarditis, and from the slighter forms of disease of the pericardium, may end in the production of local lymph-patches, or in local adhesions of greater or less extent (as described by Mr. Paget).

3. That pericarditis, ending in *considerable* adhesion, occurs at one period or other of life, in from 2 to 3 per cent. of the patients that form the hospital population of Edinburgh, and that die in hospital.

4. That *less considerable* adhesions (not clinically important) occur in about 5 per cent. more (making altogether adhesions present in about 8 per cent. of the hospital population that die from all causes).

5. That lymph-patches, chiefly on the surface of the right ventricle, or mere threads of adhesion at the extreme base (indicating the previous occurrence of morbid processes of a more local kind and of lesser intensity) occur in not less than a *third* of all the patients who die in Edinburgh Royal Infirmary.

6. That acute pericarditis, in actual progress, occurs in about 6 per cent. of the fatal cases; but that in very many of these cases it is slight, and in almost all of them subordinate to other grave constitutional or local diseases; so that primary and uncomplicated fatal pericarditis is a disease of exceedingly small mortality.

7. That the healing or repair of pericarditis by adhesion, or by lymph-patches, must be regarded as a greatly more frequent event than its fatal issue; and that the formation of lymph-patches, as the result of slight and local irritation, is one of the commonest of morbid affections.

The *treatment* of pericarditis followed by Dr. Gairdner, is founded on the vital phenomena or symptoms. The existence of a friction-sound, *per se*, he does not regard as a proof of pericarditis requiring active treatment. In the rheumatic form of the disease, to which his remarks on treatment are confined, he has never had a fatal case; and his treatment of the local affection has always been subordinate to the treatment of the general affection which it complicates. In the very beginning of some cases when pain over the heart was marked, and especially when the characters of angina were developed, he applied from four to six leeches, followed by warm fomentations, plain, or medicated by opium. Friction, with camphorated and ioduretted liniments, was sometimes had recourse to, and in obstinate cases blisters were employed. General bleeding and the use of mercury he does not approve of. The constitutional treatment he adopts is directed entirely to the removal of the rheumatism. Considering the mildness of his treatment, and his success, it is natural to infer that the *prognosis* in rheumatic pericarditis is *very favourable*.

Dr. Cockle's monograph on insufficiency of the aortic valves, in connection with sudden death, requires a shorter notice. It is divided into two parts. The first and larger part consists of

a historical sketch of the views of eminent men with regard to this disease, from the time of Harvey downwards. It appears that our forefathers, previous to the time of Laennec and Avenbrugger, did know something of this variety of heart disease. Their knowledge, however, as might be expected, was derived more from post-mortem appearances than from symptoms manifested during life. After the introduction of physical diagnosis, greater light was thrown on the pathology, as well as the diagnosis of the disease; and, as a consequence, the writings of the last thirty years are more worthy of study.

The second part of the pamphlet contains Dr. Cockle's hypothesis respecting the cause of sudden death in this disease. We may say in passing, that we did not find the study of the first part to facilitate in any way the understanding of the second. He believes sudden death is generally caused by syncope in aortic insufficiency, and his theory as to its production is nearly as follows:—The coronary arteries are injected by the systole of the aorta, and during the diastole of the left ventricle. The aortic trunk, through the constant overforce of the ventricle, becomes dilated, loses its elasticity, and generally becomes atheromatous. The mouths of the coronary arteries participate in the latter change. As a consequence, the coronary arteries are imperfectly injected. The substance of the heart, as a result, is badly nourished, and being at the same time called to do double duty from the valvular defect, a fatal issue is obvious. The suddenness of death may arise from some temporary cause, such as mental excitement or errors in diet. He believes death is also frequently hastened by the supervention of mitral incompetency in the course of the disease, and consequent embarrassment of the lungs and right side of the heart. The pamphlet is concluded by a few sensible observations on the treatment of the disease.

IV.—*Instructions to Mothers and Nurses in the Lying-in Chamber.*

By J. C. LORY MARSH, M.D., &c. Pp. 22. London: J. W. Davies. 1861.

THE size of the little pamphlet before us appears to be the main, if not the only point in regard to which it invites severe criticism. The title, as above quoted, would not unnaturally lead us to suppose that we had here another of these works of which we have lately had not a few, and which profess to treat of familiar subjects in a familiar way. Most of these productions have one fault—they are too long; some have two; they are, in addition,

inaccurate. It is thus that we have, after perusing Dr. Marsh's *brochure* of twenty pages, concluded that the author has, viewing the matter in the same light that we do, fallen into the opposite error of making his book, when taken in connection with the title, appear insignificant. Dr. Marsh, however, has evidently studied the subject with care, and the work, in so far as it goes, merits our warmest approbation.

Addressed as it is to mothers and nurses, it treats of many points in which both are deplorably ignorant. There is, perhaps, nothing in regard to which more erroneous ideas prevail than the duties of nurses—where they begin and where they end; and there is no practitioner of any experience, who cannot call to mind many instances of the insolence, ignorance, and disobedience of these women, who often look upon the doctor as a colleague, whom the prejudices of society have forced upon her. On this point our author's remarks are sensible; and his strictures on the present class of nurses are as severe as they are well-merited. No doubt there are many really good nurses, and with whom the accoucheur may leave his orders with perfect confidence; but is it not the infrequency with which such women are to be found that makes us appreciate them so highly? Why should we not have many such? The reason is only too easily found, in the utter inefficiency of all training establishments for nurses. In our hospitals, the pay of nurses is so miserable, and their duties so arduous, that it is rarely, indeed, that they are obtained but from a very low grade of society; and when they do enter these hospitals they do so to earn a means of livelihood, and not to qualify themselves for private nursing. In all our large towns a certain number of women receive instruction in the principles and practice of midwifery, and it is from this comparatively rare class that all our best nurses are obtained. But the number of these is far too limited to supply both town and country practitioners with good nurses, and they are, therefore, replaced by the numerous and objectionable class to which we have alluded, who owe their position mainly to an arrogant assumption of skill which they do not possess. The age of the great majority of the nurses is also alluded to by Dr. Marsh in remarks so sensible that we quote them for the benefit of our readers:—

"I believe it is a popular error to suppose that monthly nurses, like port wine, improve with age; or that the fact of a woman having had a *large family* is a criterion that she is therefore more experienced, and better able to attend upon her neighbours than those who have never been so bountifully blessed. So far as a tolerably extensive field for observation has enabled me to form an opinion, the best nurses I have met with, have been active, strong, and obliging young women, with hearts, and hands, and heads willing to serve their neighbours as themselves; and the worst class have been old worn-out women, whose energies have been sapped by a large family, and whose misfortunes

(not their inclination) have led them to adopt a laborious life as a means of subsistence, because they are unable to obtain a living in any other way, and not because they possess one single quality of mind or body to fit them for such an important post. Inasmuch, then, as attending upon lying-in women is of all occupations one of the most useful a nurse can engage in, it is also by far the most exhausting, the most wearying, and sometimes, physically and mentally, the most trying. I would recommend all nurses to commence this branch of their duties early in life; in fact, they should not exceed 30 years of age when they begin. If not better single than married, at any rate they ought to be unincumbered with the cares of a family. They cannot cultivate too scrupulously habits of cleanliness and neatness in their personal appearance, as well as a kindly and considerate mode of expressing themselves towards those around them; for in cases of danger, and perhaps with no dear relative near to pour in a word of heavenly comfort, it no doubt gives much more comfort to the patient when all worldly pride is laid aside. I have often felt, when life has been hanging by a thread, and all one's powers of mind and body are taxed to the uttermost, what a relief it would be to be freed from the poor, feeble, gossiping, perhaps dirty, old woman, and replace her with a young active woman, with a warm heart to sympathize, willing hands to assist, and a clear head to carry out the instructions of others. Although single women are rarely met with in this department of usefulness, I am sure their entrance into it would be acceptable to the medical man, and most beneficial to the patient. Only imagine, for one moment, a woman about to pass through a severe trial, one under which it is of the utmost importance that she should be surrounded by those who are both willing and able to minister to her every want, and not by those whose feebleness is such, that it has often struck me, when meeting them at the bedside, that they came to be *nursed* rather than to nurse!"

Dr. Marsh's object in publishing his little work is to try and improve the existing state of things, which he thinks might be easily accomplished through the agency of medical men, clergymen, ladies' sanatory institutions, &c. We sincerely wish him success, although we fear that it may not be so easily accomplished as he seems to imagine. We regret extremely that Dr. Marsh has so curtailed his work, written as it is in a style far superior to that of medical literature in general. Had he enlarged a little more on some of the topics of which he treats, he would, we think, have made his work more generally useful; but we heartily repeat our wishes for his success, and look forward to the day when the "Sairey Gamp" style of nurse will be numbered with the traditions of the past.

V.—*Operative Surgery.—Adapted to the Living or Dead Subject.*
By C. F. MAUNDER, F.R.C.S., &c. Pp. 318. London :
John Churchill, 1861.

THE author, in the preface of the work now before us, apologizes for his appearance before the public in the character of an operative surgeon, by stating that he is supplying a demand for a work

sufficiently concise and systematic for the guidance of the general body of medical students, at the same time neither pretending or attempting anything original in the production. At the present time, when so many aim at the dignified name and position of an author, we can only judge of such a work as this, by comparing it on its own merits with the works of men who have already cultivated the same field, and see whether he expresses in a plainer or more concise form those scientific questions which are now satisfactorily settled, and, consequently, in the discussion of which no originality can be expected, or whether he carefully traces up to the present time, and collects the isolated discoveries of men in the different parts of the world. In none of these particulars do we find this book to be either superior or equal to the works of Ferguson, Skey, &c., on operative surgery, or the practical part of such books as Miller, Druitt, Erichsen, and many others.

At the same time, we do not consider the book as deserving of censure on every point, for in the opening chapters we have a short dissertation on manipulative surgery, including a description of the various surgical appliances and the mode of application, which, as far as it goes, embraces as much perhaps as the majority of surgeons at the outset of their career require to be acquainted with. At the same time, we consider that the author on entering minutely into the method of fastening a bandage or reducing a dislocation, should at least have given a short description of the splints used in treating different fractures of the extremities, and their mode of application; surely this is as much manipulative surgery as some of the other things described, more especially from the frequency of such accidents, and the pecuniary risks to the medical man attending their treatment. These, however, are omitted, and a couple of pages are occupied with the details connected with the reduction of dislocations, in order that the author may introduce, what he considers an original plan, the employment of a "Hodge's accumulator" in producing extension, alleging its superiority to the ordinary manual assistance, or traction by pulleys. Such may be really the case, but we think a more extended explanation might have been given as to the manner of using such an apparatus, how the strain was to be suddenly taken off, or how the accumulators were to be stretched to the required degree without the aid of that amount of manual force, in itself more than sufficient for the reduction of the dislocation. This portion of the work concludes with a very clear description of the manner adopted in introducing sutures, tying knots, and applying ligatures, none of which probably could be better or more concisely described.

In the succeeding parts of the work, the more important operations are entered into, beginning with the mode of performing venesection, and the treatment of varix, for which he recommends the injection of the perchloride of iron. We think it would have been advisable had the author mentioned where this remedy should not be employed (as when the tumour is situated in the angle of the eye), for we have ourselves seen one instance of instantaneous death, and heard of several others, from its inconsiderate use in such cases. In the portion devoted to operations on arteries, we are recommended to use a sponge to wipe away extravasated blood. We are aware that this is still in use both in the metropolis and provinces; but we are surprised to find a recent writer recommending the application of a sponge to a cut surface. We have more than once had the impropriety of using the sponge forcibly impressed on our mind, both from actual experience and repeated warning. In the extensive hospital of this city, such an article is unknown, a substitute being easily obtained by the use of the *sponge cloth*, which as readily absorbs fluid, and can be much more easily and thoroughly washed. A piece of lint, a towel, or anything, in fact, is superior to the sponge. It is needless to occupy space in discussing that portion of the work devoted to the ligature of vessels, as the descriptions here are simple, and the anatomical details correctly stated, instructions being given for the ligature of almost all the vessels in the body.

The next section has been devoted to excisions. This opens with a lengthened account of the removal of the upper jaw. As an example of the minuteness with which, in some cases, Mr. Maunder enters into detail, and the steps he takes in the performance of the operation, we shall allow him to speak for himself:—

“The patient being seated in a chair, at a convenient height, his face turned towards the operator, and having his head supported by an assistant behind, the surgeon should stand somewhat in front, but on the side of the patient opposite to that on which he is about to operate. The operator proposes to divide the cheek from the angle of the mouth of the affected side to a point opposite to the centre of the malar bone of the same side, and effects his object either by puncturing the cheek from within the mouth (the former being made tense by the left forefinger and thumb of the operator, pulling upon the cheek on one side of the angle of the mouth, while an assistant pulls gently on the other side of the angle) opposite to the malar bone, and carrying it downwards, so as to divide the commissure of the lips; or, if the cheek be stretched over a tumour, the former may be divided from below upwards and outwards. The cheek must now be dissected upwards to the margin of the orbit, and the nasal cartilage of the affected side be separated from its attachment to the superior maxillary bone; branches of the superior maxillary nerve will of course have been cut across, as they emerge upon the cheek. Protecting the eye, divide with bone forceps the nasal process of the superior maxillary bone, opposite to the upper opening of

the nasal duct. Take now one end of a "chain saw," temporarily detached from its handle, and, by means of a thread attached to it, arm an aneurism needle with the saw, and pass the needle thus armed, from above through the sphenomaxillary fissure, bringing it out below the malar bone in front of the origin of the masseter muscle. The thread should be now seized and held, while the needle is withdrawn as it came. The saw being now adjusted and the eye protected, the malar bone will be readily divided by a few strokes of the former. Extract one or two incisor teeth from the affected jaw, separate the velum palati by a transverse incision, from opposite its attachment to the posterior nasal spine outwards to a point just behind the last molar tooth; also incise the soft palate along the side of the articulation between the palate processes of the palate and superior maxillary bones. Again arm an aneurism needle with the chain saw, and pass it back along the floor of the nostril of the diseased side, making it reappear in the opening caused by the transverse incision in the velum palati. With a forceps seize the thread and bring it and one end of the saw out at the mouth, at the same time removing the aneurism needle by the nostril. Readjust the handle of the saw, and make it cut its way forward through the hard palate. The chief osseous connections of the superior maxillary bone are now severed, and having introduced an elevator between the maxillary bones in front, slight force applied by means of the elevator used as a lever, while the sound maxillary bone serves as a fulcrum, will break down the still existing unimportant bony adhesions.

"With a strong pair of forceps holding the alveolar process, pull upon the loosened bone in a direction downwards and outwards, twisting it as it yields, so as to break down slight adhesions and to tear small vessels, using the knife sparingly, but to divide perhaps the superior maxillary nerve. Any vessel bleeding should be ligatured if it can be reached, otherwise be touched with the actual cautery.

"Bleeding having ceased, the cavity should be filled with lint or sponge, and the wound carefully closed and sutured. After recovery the patient may be handed over to the dentist.

"Any vessel bleeding during the operation may be ligatured at once.

"On examining the bone thus extracted, it will be found that, besides portions of the palate, malar, and other bones, usually a greater or lesser portion of the pterygoid process comes away also."

This may be taken as a good sample of Mr. Maunder's descriptive powers; we shall leave it to the reader to determine, whether it is sufficiently clear and concise as to enable the student to understand and perform a complicated operation for the first time. In this he advocates the use of the chain saw, which in such cases may be sufficiently applicable on the dead, but in the only case where we have seen it employed in this operation on the living subject, and by a very expert surgeon, we certainly have no desire, from what we witnessed on that occasion, to see it tried again. A pair of bone forceps does all that is required, in the first instance much more speedily and satisfactorily, and with regard to inserting the saw by the floor of the nostril, and bringing it out by the mouth, this, which in the living subject would be scarcely possible, owing to the restlessness of the patient himself, and the bleeding occurring during the operation, is at the same time quite an unnecessary proceeding,

as the arch of the palate is cut or notched through with much greater facility by a Hay's or a narrow backed saw, and the bone is then easily depressed and separated. We are glad, however, to see, in the same chapter, that Mr. Maunder recommends, and has performed excision of the head of the femur. This is an operation which, considering the great frequency of morbus coxæ, and the fatality attending it, has not been sufficiently often tried as a *dernier ressort*. The operation in itself is not one involving very great danger, more especially on the young subject, considering, as Mr. Maunder mentions, that the parts are in a state of atrophy and disease, and the head of the bone is so easily turned out. However, it is very questionable whether the limb is at any time likely to be of much use; but a more extended experience of the operation may prove that ultimately it may become of some service. In one case we witnessed, there was no doubt it saved the life of a strumous girl of thirteen who made an excellent recovery, although at the time of the operation, the acetabulum was extensively diseased; but two years after, little or no weight could be borne on the part.

In the section devoted to amputations, of which we cannot but speak in the highest terms, it being, we consider, the best part of the whole volume, and the one most likely to be of service to the student, he mentions and describes almost every mode of performing amputations, including Mr. Teale's excellent modification. We are glad to see that he has highly recommended this manner of operating. There appears to be little doubt, if we are to credit the statistics published by Mr. Teale himself, that this method is much superior to any previously practised. Not only is the risk incurred by the operation much lessened, but it greatly facilitates the adaptation of artificial limbs, and also prevents that pain and annoyance occasioned in almost every instance by the adhesion of the cicatrix to the end of the bone. Besides, there are advantages arising during the treatment of the stump, or in Mr. Teale's words:—

1st. It avoids tension.

2nd. It does not disturb the plastic process, and consequent placing of the large veins of the limb, as well as the smaller veins of the bone, in a condition the least likely to take up purulent matter, putrid blood, or serosity.

3rd. The position of the incisions allows for a free discharge of purulent and other matters.

The plates of this portion of the work are admirably executed, and a careful study of them cannot fail to give the student a most correct idea of the various operations. In the description, however, of Mr. Syme's amputation at the ankle, we do not think Mr. Maunder has entered fully enough into the cause of the difficulty

generally experienced in this operation: he says, p. 46—"It is preferable to Chopart, because certain bones which are disposed to caries, either idiopathically or in consequence of their anatomical relations, are removed. On the other hand, the flap is formed with great difficulty, and being hollow, and composed of lowly-organized tissues, secretions are liable to lodge in the pouch, and sloughing occasionally occurs." Now, it is a well known fact, or rather one which Mr. Syme has endeavoured to inculcate for a long time past, that this great difficulty which our author appears to experience is entirely owing to the operation not being properly performed. If the incision carried round the sole of the foot be made too far forward, as is frequently done under the impression of having plenty of skin to bring over the end of the stump, then it will be found extremely difficult, nay, almost impossible, with a projecting calcareum to get the flap dissected back; but, if, on the other hand, no more of the skin on the heel be taken than is absolutely necessary, the flap may be dissected back with great facility. The retraction of the flap in this case amounts to little or nothing, so that no advantage, even in this way, can be obtained by having it too far forward. With regard to the probability of the flap sloughing, we consider that this again is entirely the fault of the operator, for if, as Mr. Maunder himself remarks, in dissecting back, he divide the vessel supplying the part, which he will be very liable to do in his efforts to get too long a flap cleared off the heel, sloughing will in all probability be the result. In one case witnessed by ourselves, the vessel had to be ligatured on the table, and in a large number of such amputations which have come under our notice, this was the only one where sloughing occurred. Indeed we understand Mr. Syme never had this result in any one instance.

Mr. Maunder appears inclined to recommend Pirogoff's in preference to the previous operation, because there is less shortening of the limb; but we think the statistics of this operation will deter any one from preferring it to the other, as it cannot be denied that in a great number of these cases, although the stump was healed, it was, owing to the pain caused by the least pressure, quite useless. We understand that even the originator, after having performed the operation in the Crimea some eighty times, has found reason to discard it in favour of Mr. Syme's.

The author contrives, in the short space of five pages, to compress the whole subject of the different forms of hernia. The varieties are little more than mentioned, and the anatomical relations of the parts (without a knowledge of which an attempt to acquire a correct knowledge of the operation is hopeless) are treated with great paucity of description. Let us, for instance, notice the description of the operation for femoral hernia,

and see how far it is likely to be of assistance to the student. He says:—

“The mode of liberating a stricture must depend upon the relative anatomy of the parts adjacent to the hernia. In femoral rupture, the usual seat of stricture is at Gimbernats’s ligament, and to sever this structure, the edge of the knife must be directed inwards. The femoral ring is bounded in front by Poupart’s ligament and the spermatic cord in the male; behind, by the horizontal ramus of the pubis; externally by the femoral vein, only a thin septum of the femoral sheath intervening; and internally by Gimbernats’s ligament. Thus it will be seen that a hernia making exit at the crural ring and becoming strangulated, the stricture will be liberated with less risk of injury to neighbouring parts by nicking Gimbernats’s ligament, and, whether the sac be opened or not, by cutting *inwards*.”

In the short space of sixteen lines we have all the anatomical details and the plan to be pursued in operating. For brevity our author is certainly unrivalled; but let us see, even in this, if his account of the parts to be divided is correct. He says the stricture is at Gimbernats’s ligament, and the edge of this stricture must be divided with the knife. According to the authority of our most eminent surgeons, and with them Sir Astley Cooper, the sharp edge of Gimbernats’s ligament does not require to be divided. Sir Astley says:—“It has been recommended to cut in the direction of Gimbernats’s ligament, towards the symphysis pubis; there will be no necessity for this, as the stricture is not situated at Gimbernats’s ligament; it is never known to be there. The seat of stricture in femoral hernia is at the crural arch, just where the intestine leaves the abdomen, and when this is slightly divided, the stricture gives way, and by a little pressure the parts are easily returned. I have known Gimbernats’s ligament divided under the supposition that it was the seat of stricture, whilst the stricture itself has remained undivided and the patient died. This shows the folly of stating that Gimbernats’s ligament is the seat of stricture. Whoever dissects a strangulated hernia will have an opportunity of learning this for himself. Surgeons will find that the division of Gimbernats’s ligament will not liberate the stricture, that it will remain the same, and that the constriction is at the *crural arch*.”

Further on he observes:—“Persons who think the stricture is at Gimbernats’s ligament are grossly ignorant of its real seat, and I can scarcely tell how to express my contempt for those who resist the evidence of their senses.” Not only do we find this great authority, with an experience of nearly forty years, thus describe the parts, but other English surgeons tell us the same. Lawrence and Hay, as well as Erichsen, agree with Sir Astley on this point. The line of incision then, should be *upwards* and *inwards*, and not *inwards* alone, as our author recommends. We have ourselves made several dissections where the operation was

performed with such an incision, and where the hernia was reduced with great facility; and in these we found Gimbernat's ligament untouched, the incision being carried in a direction upwards and inwards.

In like manner, in the portion of the work devoted to inguinal hernia, no notice whatever is taken of anatomical details; what parts we would require to divide, and how many, before we reached the sac itself. We are naively told to cut down and find the stricture, without giving us any hint concerning the position in which it is to be found. This is the more inexcusable, as in a strangulated inguinal hernia the position of the stricture varies to a great extent. Sir Astley mentions no less than four different situations in which the stricture is to be met with "at the abdominal ring in large and old hernia; in ordinary cases, one inch and a half to two inches above the abdominal ring, according to the size of the hernia; the stricture is sometimes, but rarely, occasioned by a membranous band across the sac, or by a portion of omentum becoming entangled with the intestine." Further he remarks:—"If a man sit down to the operation under the supposition that the stricture is situated in the abdominal ring, he will likely destroy the life of the patient. I will tell you what he would do; he would pass the instrument on the finger up to the abdominal ring and divide it; then he would endeavour to push the intestine into the abdomen, but it would return as often as the attempt is made; and not conceiving the stricture to be at any other part, his attempts would be repeated, and the force employed would most probably rupture the intestine."

We next have the radical method of curing hernia, as introduced by Wurtzer, the one most in use and probably the best at present known, although, unfortunately, even this plan, ingenious as it is, cannot be relied upon in a large number of cases to produce a permanent cure.

In the section devoted to lithotomy, we feel again inclined to quarrel with our author for omissions. While he has entered pretty freely into the ordinary lateral operation, and into Allarton's median incision method, we have not one word said concerning an operation which is almost universally adopted in the school of this city. We allude to the rectangular, as introduced by Professor A. Buchanan, which has been in constant employment here since 1846. Mr. Maunder does indeed state that the staff used may be either curved, rectangular, or straight, but immediately afterwards tells us that the two former can be more securely fixed under the arch of the pubis, thus showing that had he possessed any knowledge of this operation, his ideas were so indistinct at the outset as to lead him into an error which would in all probability have proved fatal to its success, as the rectan-

gular staff should not be carried close under the pubis, but on the contrary it should be held under the level of the bulb of the urethra, and only when the staff is placed in that position can a safe road to the bladder, and sufficient space for extraction be obtained. That this operation is well worthy of being considered one of the best, if not the best method of cutting for stone, the results fully prove, and for the perusal of Mr. Maunder, we would recommend Dr. Andrew Buchanan's paper, contained in No. 29 of this journal. He there shows that the average number of deaths has not been more than one in twelve, while in the old operation it is as high as one in six. These results were not obtained in the hands of a single operator, but by no less than ten different surgeons, all performing the operation in this way on the living subject for the first time. Under these circumstances, we consider that a work pretending to embrace the whole subject of operative surgery, should at least, on such an important operation as cutting for stone, have mentioned the most generally applied methods. We have ourselves seen all these plans tried many times, and for rapidity and ease of execution, and safety as regards the parts divided, we consider the operation of Dr. A. Buchanan pre-eminently superior. Mr. Allarton's method may be simple when compared with the ordinary lateral operation; but it has the great objection of forcibly dilating and tearing the parts, and being exceedingly tedious in execution.

It does not fall within our province to discuss the different structures in the two, or rather three operations; however, we cannot but notice that Mr. Maunder, in his description of the lateral, recommends to cut, after entering the knife, directly downwards, "dividing probably, skin, common superficial fascia, a few fibres of the external sphincter, and some fat in the ischio-rectal space. Another sweep of the knife from above downwards, deepens the wound, dividing the deep perineal fascia, transverse perineal muscle and artery, and more ischio-rectal fat." Now, this mode of carrying the incision directly downwards, although recommended by some eminent surgeons, appears to carry with it an increased amount of danger from the almost certain division of the perineal fascia, and thus opening into that cellular space beneath the prostate, or between the bladder, prostate, and urethra on the one side, and the rectum on the other, and thus allowing urine to get into a part separated from the abdominal cavity only by a thin membrane. One of the most successful operators we ever had, viz., Cheselden, never made his incisions in this manner, but always cut the prostate outwards, keeping the edge of his knife turned up. It cannot but be acknowledged that, with the incisions recommended by Mr. Maunder, there has

an increased mortality in the hands of almost every operator. This, then, we consider the most conclusive argument that can be urged against its practice.

In concluding these remarks upon Mr. Maunder's production, we have no desire to undervalue the work, as it cannot be denied that, although on some points it is somewhat brief and unsatisfactory, yet, as a whole, it has a thoroughly practical bearing; and to any one having already had instruction on the subject, and wishing to practise his operations on the dead, its directions in the dissecting-room will be found of great service; but still we must adhere to our former opinion, that as a work embracing the subject of operative surgery alone, and having for its object the guidance of the student, to whom it is of the highest importance to be carefully instructed in all the minor details of the art, it will be found inferior to many of the works we already possess.

VI.—*Ten Lectures Introductory to the Study of Fever.* By ANDREW ANDERSON, M.D., Lecturer on the Practice of Medicine, Anderson's University, Glasgow. Pp. 180. London: John Churchill, 1861.

THOUGH we have to lament that few works have emanated from the medical men of this great city, yet we are proud of the high character of those which have appeared. Few medical works have attained the same circulation, or have been so highly esteemed as those of Burns and M'Kenzie. It was a matter of no small gratification to us when Dr. Anderson's little work was announced; but at the same time it caused us considerable anxiety lest it should fall short of the meritorious character of the treatises to which we have just referred. We therefore perused it with deep interest, and it now affords us great pleasure to express our opinion of its merits in high terms.

Dr. Anderson's reputation as a lecturer, combined with the excellent character of his numerous valuable contributions to the periodical literature of the profession, gave us an *a priori* confidence that his work would prove worthy of the Glasgow medical school, and we have not been disappointed. In his short preface, Dr. Anderson modestly tells us that:—

"The following lectures have been printed from a short-hand writer's notes of my extempore speaking, corrected and somewhat condensed. In substance they do not pretend to more than is assumed on the title-page; and if they are found useful to students, my end is gained."

We have to express our regret that Dr. Anderson followed this plan. We feel assured that the value of the work would have been very much enhanced, had it been thrown into the form of a treatise. The lecture-style detracts from its value by frequency of repetition, and at the same time prevents elegance of composition and conciseness of expression. We have no doubt but that a second edition will soon be called for, and we strongly recommend the talented author to take our hint into consideration.

In Lecture I. he discusses the causes of fever, and arranges these under the heads "of Cold; Irritation of body and mind; and Poisons, viz., malaria, effluvia, specific contagions, and epidemic influences." He next adverts to the "nature of fever, its essence and concomitant derangements."

In Lecture II. he describes the types, the forms and complications of fever. We are not prepared to coincide with his definition and classification of the types of fever. He remarks:—"We divide it [fever] thus into eight different types, or in other words eight distinct phases which fever may assume," p. 19. We beg to observe that the term "phase" appears to us to be a very unhappy one to define the meaning of type; but we deem the eight-fold classification of types not only too formal, but also opposed to clinical observation. The eight types are, 1st, Mild; 2nd, the Toxic; 3rd, the Congestive; 4th, the Inflammatory; 5th, the Asthenic; 6th, the Nervous; 7th, the Septic; and 8th, the Typhoid. We deem this an unnecessary multiplication of types, and one which is not borne out by bedside observation. The second, the third, the fifth, the sixth, the seventh, and the eighth, are all mere modifications of the asthenic; consequently, we think that it would have been more philosophic to have arranged fevers under two types, the sthenic and the asthenic. This great multiplication of divisions must, at least, confuse the student. We have much pleasure, however, in stating that Dr. Anderson's directions regarding the treatment of the different types are all that could be desired, and will afford great assistance to the young practitioner in the management of the disease. With regard to the *forms* of fever, the author observes that fever may assume five forms under any of these types. "There are eight types, one or more of which any fever may assume. Now what are the *forms* which fever may assume under any type? These are five," &c., p. 31. These five are "Ephemeral, Intermittent, Remittent, Continued and Relapsing." We are sorry to dissent again from this arrangement. That which he denominates *forms*, are essentially different diseases; not merely forms of the same affection appearing under different types. We think that, logically speaking, the matter should be presented to the student under a totally different aspect—viz., the different types under which

these different *kinds*, or species of fevers, usually present themselves. But we turn with much pleasure to another topic, namely, the complications of fever, because it enables us to express our satisfaction with the author's remarks on these important points, especially in reference to the subject of erysipelatous œdema of the glottis. We cannot do better than to quote this passage:—

"It sometimes attacks the fauces, and descends to the opening of the larynx, causing suffocation by producing *œdema of the glottis*. This is one of the most dangerous of all the sequelæ of fever. It occurs after typhus; it occurs after small-pox. Sometimes, as I have said, it follows erysipelas of the head. At other times it takes place alone. In pure œdema of the glottis the patient complains of scarcely any pain in the throat; but there is rapidly increasing difficulty of inspiration, which is stridulous, the expiration being comparatively easy: you can hear the stridulous croak at a great distance from the patient. I saw many such cases when clerk in the Fever Hospital here years ago. There was one winter when typhus was very rife. The house was full, and we had more than a score of cases of this post-febrile œdema of the glottis, and many of these people died. In some of them we found no redness of the mucous membrane, but only watery swelling of the tissue covering the glottis, which had closed the rima, and suffocated the patient. It is important, however, to observe, that the swelling is always *above* the true vocal cords. It is the tissue about the arytenoid cartilages and the upper orifice of the larynx which is swollen and infiltrated; so that you may save the patient by making an opening in the crico-thyroid space, and you do not require to cut into the trachea. Of course there are the ordinary difficulties to combat—the necessity of keeping the trachea clear of mucus which the patient cannot cough out, and that of seeing that no cold air be introduced into the lungs, so as to produce the complication of bronchitis; but these may sometimes be happily overcome, particularly if the operation be not too long delayed. I remember a medical student, convalescent in one of the side-rooms in the fever-house, whose death occurred in a most unfortunate way. He had distinctly some œdema of the glottis, some degree of the stridulous inspiration which I have described; and a consultation had been held to consider the propriety of operating. A short delay was decided upon, to see whether the disease would not yield to other remedies. In the meantime he suddenly died. He had turned round in bed in the nurse's presence, and all at once he ceased to breathe. On inspection we found that the œdematous swelling affected but one side of the glottis; and when he turned round on his other side, the flap of swollen mucous membrane fell into the vocal chink and choked him. From this case I drew the conclusion, that where the stridulous breathing is thoroughly established, and is not relieved by scarification of the swollen epiglottis, it is well to operate without delay. If you do not, the lung is apt to become congested, and your subsequent operation is not so likely to end in recovery. And yet no case is hopeless:—I recovered two patients by laryngotomy and artificial respiration, after they had ceased to breathe, and lay to all seeming dead."

We have met with several such cases in hospital practice, but, we regret to add, all terminated fatally.

Dr. Anderson's directions regarding the general management of a case of fever will be found in this lecture, and we cannot too strongly recommend this part of the work to the attention of the reader. We have only space to make one extract on a most

important point, one which has a much more extended application than that to which the author confines his remarks :—

"I must say of Glasgow—I do not know how it may be in other towns—that, unless in our best houses, the bedroom accommodation is very much sacrificed to that of the public rooms; and the sick man is very apt to be cooped up in a small airless chamber, when he might—when he ought—to be established in the best part of the house. It is of the greatest importance for his own sake, and to prevent the spreading of contagious fever, that he should have plenty of fresh air. Too often you find the sick-room filled with furniture, much as if it were an upholsterer's shop. I would recommend you in the first place to have the room cleared of everything, except what is essential to the comfort of the patient and his attendants."

In Lecture IV. he classifies fevers into simple fever, and typical eruptive fevers :—

I. SIMPLE FEVER.—1. Ordinary Ephemera and Synocha.

II. THE TYPICAL ERUPTIVE FEVERS.—2. Chicken-pox; 3. Small-pox (Cow-pox); 4. Scarlet fever; 5. Measles.

III. THE IMPERFECT EXANTHEMATA, or ordinary continued fevers of this country.—6. Typhus; 7. Enteric fever; 8. Gastric fever.

IV. THE BILIOUS FEVERS.—9. Relapsing fever; 10. Malarious fever (1. Intermittent; 2. Remittent); 11. Pestilential yellow fever.

V. THE PHLEGMONOUS FEVERS.—12. The Plague; Puerperal fever; 14. Erysipelas.

VI. THE ADYNAMIC MUCOUS FEVERS.—15. Diphtheria; 16. Influenza.

We regret that we cannot recommend this arrangement; on the contrary it appears to us that the principle on which it is founded is not a good one, and that the propriety of including several of the diseases among fevers, properly so called, is very questionable. In this lecture, Dr. Anderson ably discusses small-pox and chicken-pox. His description of these diseases is graphic, and his directions regarding the treatment admirable.

Lecture V. is devoted to scarlet fever. Its complications and sequelæ are well described, and the directions as to treatment excellent.

The first part of Lecture VI. embraces the subject of measles, and the latter part relates to typhus—the phenomena of which are well detailed; and several interesting cases are quoted from Dr. Anderson's inaugural essay on typhus.

In Lecture VII. he discusses "enteric fever," the "Fièvre typhoïde" of Louis and others. Dr. Anderson looks upon this fever as associated with disease of Peyer's glands, but admits that the local disease and the febrile excitement do not always hold a direct relation to each other. He says—"This local affection is not always, by any means, in proportion to the degree of 'fever' present. I have seen the affection of the bowels so severe as to go on to produce fatal perforation, and yet the fever

at no time arose to any height; the latter may vary, therefore, independent of the local lesion," p. 105. Dr. Anderson is a keen advocate for the non-identity of typhus and enteric fever, but it appears to us that he cannot have duly considered the above *fact*, which has been observed by many others as well as by himself. If he had done so, we think that he would have detected in this fact a very powerful argument for the identity of the two diseases. We do not intend to discuss this question here. Dr. Anderson employs the usual arguments for non-identity as skilfully as any of the writers who hold the same views; indeed, he is much more candid than many others, for he quotes some cases of *typhus*, in which unmistakable ulceration and other diseased conditions of Peyer's glands were detected at the post mortem examinations:—

"J. F., aged 27, a domestic servant, died on the eighteenth day of a fever characterized by the presence of a *very copious and livid typhous eruption* over the body and extremities. On inspection of the body, we found the mucous membrane of the duodenum covered with enlarged solitary glands, softened and ulcerated, so that in some places the peritoneum alone remained of the coats of the bowels. Peyer's glands were enlarged throughout the greater part of the jejunum and ileum, a patch in the lower part of the latter being about five inches long; and an excavated ulcer existed in its vicinity. The small and large intestines were besprinkled with numerous enlarged solitary follicles."

In order to obviate the legitimate conclusion which is deducible from such facts, he quotes a case of confluent small-pox, and another of scarlatina, in which similar diseases of the same glands existed, and triumphantly remarks:—

"Now, can we suppose it possible that small-pox, scarlatina, and typhus are *all* identical with enteric fever, because these cases prove that the special lesions of that disease may coexist with each of them? Thus, by a *reductio ad absurdum*, the position falls."

Now, we humbly think, with all due deference to our author, that the *absurdum* is all on his own side. Every one conversant with the subject is well aware that the pathological conditions of the intestines form only one of the elements in the chain of phenomena on which the advocates of identity rest their conclusion. And we cannot conceive how that, because sometimes the same lesion is found in other diseases, such as small-pox, we are called upon to discard the condition of the intestines in typhus as being of no value in reference to the solution of the relation which exists between it and enteric fever. No doubt such facts as those which he quotes, obstinately oppose the theory of non-identity, but there they are—their existence is conceded; and this is of the utmost practical moment. Dr. Anderson frankly admits the fact, but tries to avoid the inevitable conclusion, by the use of the well-known fallacy of *non-distribution* of the middle term.

Dr. Anderson also admits the co-existence in the same patient of the symptoms of typhus and enteric fevers, and quotes several cases in confirmation. Now, we are inclined to think that the logical sequence from these facts is that of *identity*, but our author denies this in the following terms:—"Their co-existence is no greater proof of their identity than a man having the misfortune to break both his arm and his leg by a fall, would be a proof that these injuries were in fact the same," p. 118. We confess that we are sorry to find Dr. Anderson employing such a mode of reasoning. The reader will find this quite an exceptional case in the work; and we feel assured had it not been for the trammels of theory, Dr. A. would never have penned such an argument. The very *illustration* which he uses is quite against his views. Unquestionably the *cause* of the fractures is *identical*; secondly, the essence or *nature* of the injuries is also identical; and lastly, the method of repair is likewise identical. All the differences which exist are solely matters of detail. Now, the advocates of *identity* contend that typhus and enteric fever originate from the *same cause*, and that the morbid lesions are similar in their essence, or nature; that the reparative processes are the same; and that the differences which really exist are merely those of degree.

We cannot conclude our remarks on this subject without acknowledging Dr. Anderson's candour and straightforwardness in bringing before his pupils so many of these important facts.

In Lecture VIII. he describes gastric, bilious, and relapsing fevers. We are disposed to question the propriety of recognizing the two former affections as fevers. No doubt febrile symptoms exist, but these arise from the morbid conditions of the mucous membrane of the stomach in the one class of cases, and of the hepatic system in the other. These affections are no more fevers, than either pneumonia or pleuritis. With regard to relapsing fever, the reader will be much benefited by the excellent description which he will find, both regarding the phenomena of the disease and its treatment.

In Lecture IX. we have brief accounts of malarious, remittent, and pestilential yellow fevers, phlegmonous fevers, plague, and puerperal fevers.

The last lecture is devoted to the consideration of erysipelas, the adynamic mucous fevers, diphtheria, and influenza. These subjects are treated in a succinct and able manner, and the treatment recommended is judicious.

In concluding our notice of this work, we can strongly recommend our readers, especially the junior members of the profession, to secure a copy. The practical directions regarding the management of the different forms and complications of the various

diseases embraced in the volume, will prove of great use to them.

We have again to express our warmest thanks to Dr. Anderson for having published a work calculated to reflect credit on the medical school of Glasgow. We regret that he has not thrown it into the form of a treatise; we again express the hope that he will do so when he sends a second edition to the press.

It will be seen that we have not hesitated to *find fault*, when we thought the talented author was in error. To have done otherwise would have been consistent neither with our duty, nor with the respect which we entertain for Dr. Anderson. We have had no intention to *flatter his work*, but to *review* it in an impartial and honest manner; and it is with the greatest pleasure that we find ourselves in the position to bestow on it our warmest commendations. Its faults are few and trivial; its merits numerous and distinguished.

SELECTIONS FROM MEDICAL JOURNALS.

EXPERIMENTS RELATING TO THE DIURETIC ACTION OF COLCHICUM. By WILLIAM A. HAMMOND, M.D., Professor of Anatomy and Physiology in the University of Maryland.

IN the Proceedings of the Academy of Natural Sciences of Philadelphia for November, 1858, I gave the results of a series of investigations relative to the diuretic properties of digitalis, juniper, squill, and colchicum, by which it was shown that the latter alone possesses the power of increasing the amount of organic matter eliminated by the kidneys. From this circumstance the argument was adduced that this substance, of all those experimented with, was the only one that could be regarded as a true depurator of the blood.

The results obtained by earlier investigators cannot be regarded as satisfactory, owing to the faulty manner in which their analyses were made. The urine was concentrated by heat, and thus a large quantity of its organic matter underwent decomposition.

Since the publication of my experiments, Dr. Garrod of London has studied the physiological action of colchicum; but led away by his theory of the nature of gout, he limited his researches mainly to the termination of its influence over the excretion of uric acid, which, as is well known, forms but a small proportion of the total amount of organic matter excreted by the kidneys. As the result of his investigations, he announced that colchicum does not increase the quantity of uric acid contained in the urine, and that it is not by any action on the kidneys that the remedy in question exerts its curative influence in gout. His result, as relates to the uric acid, does not, so far as I know, conflict with mine, as I did not separately determine the quantity of this substance present; but his conclusion that colchicum is not a diuretic in the true sense of the term, is certainly not borne out by his own experiments, and is directly at variance with those which I performed.

It was, therefore, obviously necessary that additional investigations should be instituted, and I accordingly undertook the task of furnishing further contributions to the subject. Before proceeding to detail these, I desire to call attention to the valuable memoir of Prof. Austin Flint, in the number of this journal for November, 1860, entitled, "Clinical Researches on the Action of Diuretic Remedies." In this essay, in addition to much other valuable matter, the conclusion at which I had arrived relative to the action of colchicum is confirmed; Professor Flint finding it to produce a marked increase in the amount of solid matter eliminated by the kidneys, without, however, increasing the quantity of water of the urine.

The investigations to which the present paper relates consisted of experiments upon adult males, in a good condition of health. In all cases, the official tincture of the seeds of the *colchicum autumnale* was given.

The determinations made were the following:—1st, the quantity of urine; 2nd, its specific gravity; 3d, the total amount of solid matter; 4th, the quantity of inorganic matter; 5th, the quantity of organic matter; 6th, the amount of uric acid.

The quantity of urine was determined in cubic centimetres.

The specific gravity was ascertained by means of the specific gravity bottle and a delicate balance.

The total amount of solid matter is given in grammes, and was determined in the following manner:—Ten cubic centimetres of the urine were evaporated to as complete dryness as possible *in vacuo* over sulphuric acid, and the residue accurately weighed. By simple proportion the amount of solids in the whole quantity of urine was easily ascertained.

Although it is impossible to get rid of all the water by this process, the quantity remaining is extremely small, and the results obtained are far more accurate than those obtained by evaporating to dryness in the water-bath, as generally practised. No matter how carefully this latter process is conducted, the loss of urea by decomposition is always an important item, and involves far more serious errors than the imperfect desiccation by the former process.

For the determination of the amount of organic and inorganic matter separately, the solid residue obtained as above was mixed with ten or fifteen drops of moderately strong nitric acid, and gently heated till the mass was well dried. The heat was then gradually raised till all the carbon was consumed, and the mass in consequence became white. It was then cooled *in vacuo* over sulphuric acid, and weighed. The inorganic matter was thus determined, and the loss showed the proportion of organic substance.

The quantity of uric acid was determined by adding chlorohydric acid to a known volume of urine.

The first experiments were instituted upon myself. In three days immediately preceding their commencement, the average quantity of urine for each day was 1425 cubic centimetres, of specific gravity 1021·73. The average amount of solid matter was 64·28 grammes; of which 30·18 were inorganic, and 34·10 organic substance. The average amount of uric acid excreted for each period of twenty-four hours was 0·77 gramme.

During the experiments with the colchicum, my manner of living was not materially altered from that of the three days above referred to; i.e., I ate the same food and took the same amount of exercise, and endeavoured to make all the collateral circumstances the same, so as to ascertain as nearly as possible the exact effect produced by the colchicum.

First Day.—On this day I took one fluid drachm of the tincture three times—at 8 A.M., 2 P.M., and 10 P.M. The total quantity of urine excreted was 1685 cubic centimetres, of which the specific gravity was 1021·50. The total amount of solids was 70·15 grammes, of which 30·90 were represented by inorganic, and 39·25 by organic matter. The quantity of uric acid was 0·81 gramme.

Second Day.—One and a half fluid drachms of the tincture were taken, as on the previous day. Quantity of urine, 1720 cubic centimetres; specific gravity, 1020·87; total solids, 75·29 grammes; inorganic solids, 32·44 grammes; organic solids, 42·85 grammes; uric acid, 0·69 gramme.

Third Day.—Same quantity of colchicum taken as on previous day. Quantity of urine, 1784 cubic centimetres; specific gravity, 1022·57; total solids, 80·13 grammes; inorganic solids, 35·11 grammes; organic solids, 45·03 grammes; uric acid, 0·82 gramme.

Fourth Day.—On this day the quantity of colchicum was reduced to half a fluid drachm, taken as before. Quantity of urine, 1540 cubic centimetres; specific gravity, 1023·17; total solids, 69·23 grammes; inorganic solids, 31·09; organic solids, 38·14 grammes; uric acid, 0·78 gramme.

Fifth Day.—On this day the quantity of colchicum was increased to one and a half fluid drachms of the tincture before mentioned. Quantity of urine, 1698 cubic centimetres; specific gravity, 1023·68; total solids, 76·14 grammes; inorganic solids, 33·26 grammes; organic solids, 42·88 grammes; uric acid, 0·76 gramme. On this day there was some derangement of the general health, manifested by increased heat of skin, fever, and severe abdominal pains. There was also a little diarrhœa. The experiments were, therefore, discontinued.

From an examination of the results obtained by the foregoing investigations, the effect of the colchicum upon the urinary excretion cannot fail to be perceived. The conclusions which I think may be formed are—1st, That the colchicum increases the quantity of urine. 2nd, That it increases the total amount of solid matter eliminated. 3rd, That this increase is mainly due to an augmentation of the organic matter. 4th, That the amount of uric acid does not appear to be affected.

These conclusions are rendered much more probable from the fact that on the fourth day, when the quantity of the tincture of colchicum taken was reduced one-third, the effect upon the urine was less decidedly marked; and that when, on the fifth day, it was again augmented to a drachm and a half, the urinary excretion was materially increased in quantity, and the solids, the organic especially, remarkably raised in amount. The relation of cause and effect would therefore appear to exist; and accordingly it would be contrary to the principles of sound reasoning to assert that the change in the composition of the urine was accidental. It is doubtless true that the urine changes greatly from day to day, and even from hour to hour; but this fact is due to the other fact, that we are constantly varying our food, exercise, &c. When, however, as in the investigations cited in this paper, these circumstances are fixed, and only one difference exists between the ordinary mode of living and that practised during the continuance of the experiments, we are fairly justified in attributing any change in the urine or in any other excretion to the influence produced by that difference.

In the next series of experiments the effect is just as directly shown, though, for reasons beyond my control, they were not continued as long as was desirable.

The subject of these experiments was a young man 23 years of age, and weighing about 140 lbs. Before taking the colchicum, I examined his urine whilst he was taking a fixed quantity of food and exercise, he being at the time an attendant in the hospital under my charge. As the results of these examinations for three consecutive days, I obtained the following as the averages for each day:—Quantity of urine, 989 cubic centimetres; specific gravity, 1020·14; total solids, 51·20 grammes; inorganic solids, 22·45; organic solids, 28·75; uric acid, 0·47 gramme.

First Day.—On this day one drachm of the tincture of colchicum was taken three times. The effect upon the urine was as follows:—Quantity, 1021 cubic centimetres; specific gravity, 1024·18; total solids, 63·25 grammes; inorganic solids, 23·57; organic solids, 40·68 grammes; uric acid, 0·59 gramme.

Second Day.—One and a half drachms of the tincture were taken three times, as previously. Quantity of urine, 875 cubic centimetres; specific gravity, 1026·11; total solids, 60·25 grammes; inorganic solids, 20·38 grammes; organic solids, 39·87 grammes; uric acid, 0·51 gramme.

On this day diarrhoea was produced. This was of quite a severe character, and, in consequence, the colchicum was not further continued.

The remarkable effect of the colchicum in increasing the amount of organic matter excreted is, however, very decidedly shown. This increase is so great as to render the probability of its being accidental extremely small, and we cannot do otherwise than regard it as being directly due to the influence of the colchicum.

The details of the third case in which the colchicum was given have been unfortunately mislaid. I am, however, enabled to state with certainty, that the same well-marked effect over the amount of organic matter excreted by the kidneys was exerted as in the cases, the particulars of which have been given in full. The experiments were continued for six days, with variable quantities of the tincture.

What are we to infer from these investigations? It appears to me that the conclusion must be admitted that colchicum is a true depurator of the blood, and hence we have an explanation of its good effects in those blood diseases, gout and rheumatism.

It is seen that no constant effect was produced upon the quantity of uric acid eliminated, and hence these experiments do not conflict with those of Dr. Garrod. We are not, however, bound to admit that the presence of uric acid in the blood in increased amount, during a paroxysm of gout or rheumatism, is the cause of that paroxysm; and, consequently, because colchicum does not increase the quantity of this substance found in the urine, we are not to suppose that the remedy in question does not exert its influence through the kidneys.—*American Medical Monthly*.

II.—ON SACCHARINE FERMENTATION IN THE MILK WITHIN THE FEMALE BREAST.

By GEORGE D. GIBB, M.D., M.A., M.R.C.P.

The discovery of vibrios in human milk, by Vogel, was announced in a paper published in *Schmidt's Jahrbucher* in 1853. He clearly proved that these animalcules were developed within the mammary gland, from the fact of their being seen in the milk on the instant of withdrawal. He believed them to be the result of fermentation in the milk itself, the result of congestion and increased heat in these organs, connected with general excitement of the sexual system. Vogel's theory of their formation was combated by the observation, that, as the milk containing them was either alkaline or neutral, and not acid; were there fermentation, it was argued, the evolution of lactic acid would immediately destroy the infusoria.

In the latter part of 1854, I was induced to make some researches into this important question, from the circumstance of an infant being brought to me, seven weeks old, in the most extreme state of emaciation, whose mother had the appearance of the most perfect health. The child, although merely skin and bone, was healthy and plump at birth, and on very careful examination no disease could be discovered. It had never been satisfied with the large amount of milk it received, but was ravenous, and had to be spoon-fed besides. This was a first child, and the mother seemed the *beau-ideal* of an excellent and

healthy nurse. The child had no diarrhoea, but the most profuse diaphoresis and diuresis had worn it to a shadow.

What was the cause of this? An examination of the milk, carefully made, at once furnished the clue. It was rich in cream, neutral, sp. gr. 1032, and showed the presence of a large quantity of sugar. So far it seemed normal. Examined under the microscope with a high power, 7 hours after withdrawal, it revealed myriads of living animalcules, those indeed known as *vibrio baculus*, but which I venture to change to *vibrio lactis* as more appropriate. These were, to my mind, unmistakably the result of fermentation of the saccharine element in the milk, and might be owing to the large quantity of sugar present; but whether occurring in or out of the breast I had yet to determine. The next day I examined the milk as drawn from the breast, and found the *vibriones* present as before, incontestably proving that the fermentation took place within the gland, as I had previously believed. There was an absence of mammary congestion and heat, but much sexual excitement, which it became necessary to control by moral and suitable medical treatment.

I did not altogether order the child to be weaned, but prescribed a diet with plenty of good cow's milk, and occasionally the mother's milk, which it did not seem prudent to stop altogether. From this time the improvement began; the extreme action of the skin and kidneys ceased, the secretions became normal; and in a few weeks the child had become fat and hearty, and after a time was wholly weaned. The mother's condition also improved; but the milk always remained neutral, its specific gravity varying from 1032 to 1035, very rich in sugar, and containing the animalcules for some weeks. The richness of the milk became less as the child was gradually weaned, when it assumed a bluish tint. As quantities of it were drawn artificially, I had many opportunities of examining it with other specimens of milk; and the general result of my experiments went to prove the presence of a large amount of sugar, and that it turned sour much sooner than cows' or healthy human milk.

From 1854 up to the present time I have examined many hundreds of specimens of human milk, chemically and microscopically, and have occasionally found two genera of animalcules to be present, in that secreted from the glands of those whose general health was disordered from various causes during lactation, or where the process of lactation itself was unusually prolonged, or again where the quantity secreted was small and wholly insufficient to satisfy the wants of the infant. In some persons, at an early period of lactation, wherein the supply of milk was abundant and rich, and where the constitutional symptoms were similar to those mentioned in the case I have briefly referred to, the two varieties of animalcules were present, but not in the same individual. These creatures consisted *firstly* of the true *vibrio lactis*, as I shall call it, resembling little rod, or minute hair-shaped bodies, and similar to those found in some of the other fluids of the body, a minute description of which it is not necessary that I should here go into; and *secondly* of monads, which I have found to be far more frequent and common than vibriones, their diameter varying in different specimens of milk, but ranging from the 3000th to the 5000th parts of an inch. I am not aware whether they have been before discovered in milk, at any rate I purpose calling them *Monas lactis*.

Now, with regard to the fermentation within the breast, it may be observed, that this process seems, to my mind, to be a fact that cannot be disputed nor ignored by any of the objections which have been brought forward elsewhere against it. This act I would place in the sugar itself, as, indeed, the only element likely to produce it; and I hold the opinion that the act of fermentation of the sugar need not necessarily give rise to the formation of lactic acid. Admitting that it may even do so, the lactic acid thus formed would exist in too small a quantity to produce an acid reaction in comparatively such a large

quantity of milk, in some cases. And as the milk has never been acid in any single instance in which I have had the opportunity of examining it, I infer, that there must be either no lactic acid at all, or that its quantity must be exceedingly minute. And again, these animalcules are commonly found in alkaline fluids in other parts of the body, and if the evolution of lactic acid went on to any extent, they would be destroyed. In some experiments performed by Berthelot (recently detailed in the *Comptes Rendus*), he was enabled actually to produce fermentation of cane sugar in an alkaline liquor, which entirely excluded the influence of an acid. The rapidity with which the milk containing these animalcules is decomposed and turns sour, after its withdrawal from the breast, generating a large quantity of lactic acid, is an additional proof of fermentation having commenced within this gland, which at first gives rise to their vitality, and then their destruction by the subsequent chemical changes which the sugar undergoes, in which lactic acid now plays an important part.

The process of fermentation within the body, and I may also say out of it, is as yet so little understood, that I may be excused if a more rational explanation has not been afforded for its occurrence in the breast than that I have ventured to give. The glucogenic function plays such an important part in the animal economy, especially in relation to histological phenomena, that it seems to be the one at fault in this condition of the breast. To use the words of Bernard, "as soon as it fails to be supplied, epithelium is no longer produced; various diseases are the immediate result; and, under similar circumstances, life is inevitably brought to a close."*

It has occurred to me that these animalcules may be generated from the surface of the mucous membrane of the lactiferous tubes, by the fermentation of the sugar at the moment of its secretion from the blood, and hence the explanation of the large number, of monads especially, found in the milk thus affected, they are sometimes adherent and clustered around the milk globules. The necessary connection which subsists between the mammary glands and uterine organs in the body, satisfactorily explains the existing influence of the latter upon the former, in producing much heat and internal congestion through reflex nervous agency; these glands become morbidly irritated as it were, and cause slight fever. But this is usually at the early periods of lactation only, although I have seen it in prolonged lactation, when not unfrequently the vision becomes impaired, and in one female thus affected, a shade of yellow was seen by one eye, and green by the other.

In conclusion it remains for me to add, that when an infant is observed to become extremely emaciated, accompanied by copious exudations from the skin, intestinal mucous membrane or renal organs, separately or combined, and if the mother is apparently healthy, with a good supply of milk, examination becomes a matter of urgent necessity, and if it is found to contain any infusoria, it must be gradually dispensed with, and such measures adopted as shall arrest the starvation of the child.—*Archives of Medicine*.

III.—INVERSION OF THE WOMB OF LONG STANDING REDUCED BY PERMANENT PRESSURE.

The following particulars are extracted by the *Gazette des Hôpitaux* from the pages of the *Deutsche Klinik*.

A primiparous woman, aged twenty, was delivered with the forceps, in December, 1852, after 24 hours' ineffectual labour, and the after-birth was immediately extracted, several coils of the funis being twisted round the neck

* Med. Times, 3rd March, 1860.

of the child. The patient became unconscious, although the hæmorrhage does not appear to have been unusually considerable. For a long time after her confinement, the young woman continued an invalid, and suffered from paralysis of the bladder and swelling of one of the lower extremities; for six years she was subject to daily hæmorrhage from the womb, in addition to menstruation, which regularly recurred every six weeks. Various methods of treatment were unavailingly resorted to, and in August, 1857, she was introduced to Mr. Brockendahl, who, to his great surprise, ascertained the existence of an inversion in the womb of the second degree. The uterus formed in the vagina an elastic pear-shaped tumor about $2\frac{1}{2}$ inches in length, and pressure with the finger induced no other effect but an increase of the sanguineous discharge. A thin circular fold of the vagina closely embraced the womb, and might on superficial examination have been mistaken for the cervix. The speculum was inserted, and allowed of the inspection of the dark red mucous lining of the viscus from which blood was seen to exude. The vagina and all the other mucous membranes of the patient were pale, and testified to her state of confirmed anemia.

The displacement, in all probability, had taken place gradually; and as no sign of peritonitis had ever been present, Mr. Brockendahl determined, although the inversion was complete and the viscus thick and unyielding, to make an effort at reduction. After reiterated warm baths, he attempted to insert the entire hand into the vagina, but without success, the external aperture of the duct being much contracted by a scar consequent upon laceration of the perineum. The treatment was then discontinued, and the patient was lost sight of until October, 1858, when the surgeon, dreading the use of rigid instruments, each day endeavoured to soften the womb by kneading it with the hand, in hopes that its texture, thus rendered more yielding, might admit of the fundus being re-inverted. Mr. Brockendahl was, however, compelled by the reappearance of the menses to discontinue these manipulations, and he again lost sight of his patient for some weeks.

In November, having read an account of Dr. Tyler Smith's case, he resolved to try the effects of continuous pressure; one of Braun's india-rubber pessaries was accordingly inserted into the vagina on 27th November. This appliance was removed daily, for the purpose of judging of its action, replaced after being filled with water, and further distended to the utmost by insufflation. The instrument gave rise to no symptom beyond a little uneasiness, which lasted a couple of hours after its reintroduction. On the 2nd of December, as some pain in the abdomen was complained of, the pessary was extracted, and the organs were examined. Mr. Brockendahl was much astonished to find that the inversion had disappeared, the cervix admitted of the insertion of three fingers, and its labia were well defined. The womb was measured and was found to exceed, by more than half an inch, its usual dimensions. The cold douche soon restored the viscus to its natural size, and the hæmorrhage, which had lasted so long, ceased, and has not since returned.

IV.—ON THE VALUE OF ANÆSTHETIC AID IN MIDWIFERY.

By CHARLES KIDD, M.D.

We extract the following from Dr. Kidd's elaborate paper:—

As to the exact mode or manner of administration of chloroform or ether in an ordinary case of natural labour, little need be said. In the sick-room, surrounded by anxious friends, we should avoid a strange display of tubes and inhalers. There is something repulsive, too, in the idea that they have been

used by one patient after another, the previous patient possibly a case of infectious puerperal fever; nor can we so well follow with our left hand the various movements of the head, with tubes and inhalers, as with a simple piece of sponge, the patient's lips having been previously greased with oil. It is a good plan, in fact, and by its simplicity inspires confidence, to direct the patient to smell or to apply the sponge or folded handkerchief herself. The poor woman sniffs at the handkerchief all round, to catch the least trace of the chloroform odour, which, she says, is delicious, and asks and begs for more. But with the strange tubes and inhalers she is alarmed, and has a disposition only to gasp and struggle; the inhaler, in fact, covers up the mouth and nose, as the patient struggles about in the bed, and if the attendant, as I have been dozens of times, is by himself, and watching the pulse of the patient, guarding with his right hand the perineum, and only one (the left arm) free to open his chloroform bottle or apply the sponge, or snap it away, if necessary; if the attendant has to give his careful attention to the case also, and to keep his countenance composed, as if nothing was really going on, he will be unencumbered with complex tubes, and silver bottles the size of a balloon.

OBSTETRIC OPERATIONS.—I have administered chloroform in a large number of cases of other operations besides those immediately connected with labour, viz., the often tedious and very trying procedure for curing vesico-vaginal fistula, various ovariectomy operations, ruptured perineum cases, enucleation of uterine tumours, &c.—a field of practice as new and scientific, perhaps, as that for resection of joints in general surgery; almost as great a public blessing, and as much if not more entirely due to the discovery of anæsthetics. We have had, for instance, an opportunity of mentioning and watching thirty cures of cases of vesico-vaginal fistula in the practice of one operator alone; whilst in about a dozen of ovariectomy cases the operation, as now done, with "all appliances and means," appeared, as it now is represented to be, less dangerous than hernia operations. There is no class of cases, in fact, that bears chloroform so well; so that more recently the formidable proceeding for removing extra-uterine foetal growths by operation, as well as Cæsarian sections, all under chloroform, and all with fair chance of success, have been restored to our books on surgery.

Why encourage pain? It is curious that the larger number of over a hundred deaths from chloroform in general surgery has been, not from shock, or hæmorrhage, or heart disease, but from apprehension of pain—fear of pain, but not actual "shock" of pain, or deep narcotism.

Of this apprehension of pain what says general surgery? Dupuytren believed pain is letting out of vital force, as injurious sometimes as hæmorrhage, or a letting out of blood. We, as medical men, as a matter of business, become accustomed to pain; but does our patient do so? I believe ovariectomy a less dangerous operation, *quoad* chloroform, than tooth-drawing; in one we have relief of long-standing misery, in the other apprehension and chance of syncope.

Our mission as obstetric practitioners is one of tenderness, of thoughtfulness, of trust. Better far is it to lessen this pain, and apprehension of pain, to lean to the side of gentleness, and help, and health for our patient, in these hours of pain and anguish, than to neglect this apprehension of pain and stand upon the ancient ways of our ancestors—that of allowing the patient to become exhausted. There can be very little doubt, I think and admit, that gestation ending in labour is a natural, an essentially natural process; wonderful for its completeness and beauty in a physiological point of view. We have often to meet cases of labour however, that exceed the line of nature, and are unnatural and morbid.

—*Transactions of the Obstetrical Society of London*, vol. ii.

V.—EMULSION OF COAL TAR.

Dr. Démeaux of Puy l'Eveque, has addressed a note to the Academy of Sciences of Paris, on the emulsion of coal tar as a medicinal and hygienic application.

The facility of its preparation, its cheapness, the quantity of coal tar which it contains, and the great solubility of the preparation in water, recommend it highly. It is thus prepared:—

R. Coal tar.

Soap.

Alcohol—of each equal weights.

Heat in a sand bath until perfect solution is effected.

On cooling, a perfect soap is formed, very soluble in water, and forming with it a stable emulsion.

This preparation is susceptible of several useful applications either in hospitals or dissecting rooms, or as a disinfectant and deodorant.

Coal tar mixed with soap and alcohol, in soluble proportions, becomes one of the most useful articles of the materia medica. The mixture can be used in different degrees of solution. Its great solubility in hot or cold water prevents it staining the skin, cloths, or the clothes.

It may be employed in form of baths, and will be found to produce the best effects in some skin diseases. It may be also used as a lotion or fomentation, as a modifier of deranged cutaneous action, or as a topical disinfectant. It is serviceable also in destroying the odours of foetid excretions or dejections.—*Journal de Bourdeaux.*

VI.—CHLORATE OF POTASSA IN GONORRHŒA.

Dr. Irwin, United States Army, stationed at Fort Buchanan, says—

"It may not be amiss to place on record my testimony in favour of the use of chlorate of potash as a therapeutic agent, which I have constantly used during the last two years in the treatment of gonorrhœa, both in my public and private practice, with the most gratifying results. I have found it to be such an admirable remedy that I seldom resort to any other in the treatment of urethral inflammation. My method of using it is as follows:—One drachm of the salt dissolved in eight ounces of water, of which an injection is given every hour for twelve hours, at the end of which the discharge had become changed and diminished, allowing the remedy to be gradually discontinued until the second or third day, when the disease will be generally found to have ceased. So efficacious has this remedy proved in my practice, that I seldom deem it necessary to give any other medicine, save a Seidlitz draught or a dose of Epsom salts."—*Medical and Surgical Reporter.*

MEDICAL INTELLIGENCE.

1. *Quarterly Report of the State of Disease in the Glasgow Royal Infirmary for the Quarter ending 23rd June, 1861.*—The number of patients remaining in the medical and surgical wards on the 31st March was 303, the number admitted till the 23rd June was 894, so that the total number treated was 1197; of these 828 were dismissed cured and relieved, and 82 died, and 287 remained on the 23rd June.

In the fever wards, the number of patients remaining on the 31st March was 58; the number admitted till the 23rd June was 181, so that the total number

treated was 239; of these, 171 were dismissed cured, 31 died, and 37 remained on the 23rd June.

The rate of mortality has been 1 in 14½ in the medical and surgical wards, and about 1 in 8 in the fever wards.

The number of accidents admitted was 137.

2. *Opening of the new Surgical Hospital, Glasgow.*—The new Surgical Hospital, in connection with the Royal Infirmary, was formally opened on Tuesday, 21st May, at one o'clock, in presence of the directors, the medical and surgical staff, and a considerable number of gentlemen, supporters of the institution. A detailed account of the new hospital, its construction, ventilation, &c., was given in the January number of the *Glasgow Medical Journal* in an original article on the Construction of Hospitals, by Dr. M'Ghie, by a reference to which all particulars with regard to these points may be ascertained. The hospital was, for several days, thrown open for the inspection of the subscribers and the public.

3. *Special Ward for Diseases of Women in the Edinburgh Royal Infirmary.*—A ward has lately been set aside, in the Royal Infirmary of Edinburgh, for the diseases of women, in compliance with the prayer of a petition from the students. Dr. Matthews Duncan has been appointed physician.

BOOKS RECEIVED.

Transactions of the Obstetrical Society of London. Vol. ii. Pp. 368. London: Longman, Green, Longman, and Roberts, 1861.

Operative Surgery, Adapted to the Living or Dead Subject. By C. F. Maunder, F.R.C.S., &c. Pp. 318. London: John Churchill, 1861.

"On Supporting the Perineum." By Dr. Graily Hewitt. Pp. 70. London: Churchill, 1861.

Instructions to Mothers and Nurses in the Lying-in Chamber. By J. E. Lory Marsh, M.D. Pp. 22. London: J. W. Davies, 1861.

"Ourselves, our Food, and our Physic." By Benjamin Ridge, M.D., F.R.C.S. Pp. 202. London: Chapman and Hall, 1861.

On the Modern Pathology and Treatment of Venereal Disease. By P. Heron Watson, M.D., F.R.C.S. Edinburgh, 1861.

Ten Lectures Introductory to the Study of Fever. By Andrew Anderson, M.D., Lecturer on the Practice of Medicine in Anderson's University, Glasgow. Pp. 180. London: John Churchill, 1861.

The Errors of Homœopathy. By Dr. Barr Meadows. Pp. 45. London: Henry Renshaw, 1861.

Lectures on the Diagnosis and Treatment of the Principal Forms of Paralysis of the Lower Extremities. By C. E. Brown-Séquard, M.D., F.R.S., &c. Pp. 118. London: Williams and Norgate, 1861.

Lectures on Diseases of the Kidney and Dropsy. By S. J. Goodfellow, M.D. Pp. 306, plates. London: Robert Hardwicke, 1861.

Another Letter to a Young Physician. By James Jackson, M.D. Pp. 179. Boston: Ticknor and Fields, 1861.

Metanoia: A Plea for the Insane. By Henry M'Cormac, M.D., &c. Pp. 16. London: Longman, Orme, & Co., 1861.

ERRATA IN APRIL NUMBER.

Page 7, line 23, for "*absente febre*" read "*urgente febre*."

" 9, " 20, for "*Zij*" read "*Dij*."

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ORIGINAL COMMUNICATIONS.

- I.—*Illustrations of Puerperal Diseases.* By R. UVEDALE WEST, M.D., Fellow of the Royal College of Surgeons, Edinburgh; Vice-President of the Obstetrical Society of London, &c., &c.

(Continued from page 151.)

OF DIARRHŒA AS A VARIETY OF PUERPERAL FEVER.

The frequency with which diarrhœa occurs as a symptom in puerperal fevers, added to the difficulty sometimes experienced in checking it by means of astringents, while a single dose of castor-oil will occasionally arrest it at once, furnishes an argument in favour of the notion, that nature may occasionally seek to make a discharge from the intestinal canal a vehicle for the removal of a morbid poison from the system. On such a view it may be doubtful whether it is right to administer astringents in these diarrhœas at all; but we must not allow our patients to sink before our eyes, as they probably would from the exhausting effects of the purgings alone. Such a case as the following is to the point; my note of it is a short one:—

CASE XX.—DIARRHŒA.

Mrs. M. G. of M., the wife of a labourer, confined of her third child after an easy natural labour, Feb. 2, 1845. On the third day a very troublesome form of diarrhœa came on. The nurse gave her a dose of castor-oil, and the next day I was sent for. She was having profuse liquid bilious motions every half-hour.

A common astringent mixture—chalk, aromatic confection, and laudanum in peppermint water—gave no relief whatever, for the following day I found her as bad as ever; the pulse 150, small and weak, with great pain and restlessness. I added tincture of catechu to the mixture. In the evening there was febrile reaction, the pulse being fuller, the diarrhœa as frequent as ever. Gave some kino in powders along with the mixture. The medicine now began to make her sick. The next day, she being no better, I gave her a starch clyster with laudanum, and added some ext. hæmatoxyli to the mixture. In the course of this day there was some slight abatement of the purging, there being one interval of three hours. But the following day she was if possible worse than ever, although taking a dose of her medicine every two hours. I gave some more starch clysters with laudanum and decoction of logwood in them. These she could not retain a single minute. I now tried the following:—

R. Cupri sulphatis	gr. xij.
Quin. sulph.	gr. x.
Pulv. opii	gr. v.
Ext. gentianæ q. s. ut ft. pil. xij.	
Quorum sumat unam alternis horis.	

She took eight or ten of these pills as directed, with considerable advantage, but grew tired of them, preferring the chalk mixture, probably because she felt a more cordial effect from it. The diarrhœa now gradually abated, although it continued in a mitigated form about a week longer. The pulse got down to 100 on Feb. 9th, the fifth day of the disease, and the eighth of the lying-in; and on the 10th Feb. it was 85; after which she improved steadily. The milk and the lochia had become suppressed. The former returned.

The following case is a better illustration of diarrhœa as a variety of puerperal fever; there was slight metritis accompanying it:—

CASE XXI.—DIARRHŒA.

M. O., an unmarried primipara, confined after an easy natural labour, June 27, 1850. At 3 a.m., June 30th, I was summoned hastily; she had had a shivering fit followed by pain and diarrhœa. I found her with a pulse 160, soft and throbbing; fever, headache, delirium, tenderness of body of uterus, white tongue; stools frequent, abundant, and very offensive. The after-pains had been severe. I gave her calomel, gr. viij. with pulv. rhei, gr. x. in a powder, ordered fomentations to abdomen, and sent

an ordinary astringent mixture, to be commenced when the powder had acted. At 2 p.m. same day, delirium and pain better; pulse 125; diarrhoea somewhat abated. Continue medicine. At noon the next day, she was much better; pulse 108; no delirium; no headache; although the uterus was still tender to touch: there was some milk in the breasts and a slight lochial discharge. She had had considerable lochial discharge from the first until the access of febrile disturbance, when it had become arrested. On the following day the pulse was below 100; and so she speedily recovered.

CASE XXII.—DIARRHOEA.

Mrs. J. S. of Alford, the wife of a blacksmith, confined, after an easy natural labour, of her second child, Sept. 23, 1854. She was attacked with diarrhoea on the 28th, with fever and a pulse 120 in the minute. I gave her an ordinary astringent mixture.

29th—Diarrhoea somewhat abated; pulse 120. It returned in the evening, with a hot moist skin and the pulse 140.

R. Cupri. sulph.	gr. vj.
Pulv. opii	gr. iij.
Confect. rosæ q. s. ut ft. pil.	vj.

R. Spir. æth. nitric.	℥ss.
Acid. sulph. dilut.	℥ij.
Decoct. quercus ad	℥vj. M.
Sumat coch. ij. et pil. i.	quartis horis.

30th, A.M.—Pulse 120; diarrhoea several times in the night. Continue medicine.

Evening—Pulse 140; more fever again; bowels better; has been very sick. Milk going.

R. Acid hydrocyanic. (Scheele.)	℥xvj.
—sulph. dilut.	℥ij.
Æther. chloric.	℥ij.
Spir. æther. nitric.	℥ss.
Aquæ ad Oss.	M. S. c. ij. quartis horis.

Oct. 1st—Diarrhoea six or seven times during the night; pulse now 110; no fever; sick at times; throws up her medicine. To take acetate of lead and opium in pills, discontinuing the mixture.

Evening—There is pain and sickness; pulæ 120; skin moist; diarrhœa better.

R. Magnesiæ calcin.	℞j.
Acid. hydrocyanic.	℞xij.
Liq. morph. acet.	℥ij.
Tinct. aurantii.	℥ss.
Aquæ ad Oss.	M.
S. c. ij. quartis horis, cum singulis pilulis.	

Oct. 2nd—Better; pulse 110; no evening exacerbation; sickness gone; diarrhœa well. Discontinue pills.

4th—Pulse 90; no complaint; milk returning; and so she recovered.

CASE XXIII.—DIARRHŒA.

Mrs. J. G. of T., the wife of a shoemaker, and the mother of several children, confined June 16, 1858. The vectis was used. She was doing well until June 26th, when she had a shivering fit, followed by fever and diarrhœa. The next day, when I saw her, the pulse was 120, small; diarrhœa profuse; great pain; headache; tongue white, moist; lochia and milk checked. I gave her chalk mixture, with laudanum, &c. The diarrhœa was checked by that medicine, but returned the following morning with greater violence than ever. I added decoct. quercûs to the mixture.

June 29th—Better; pulse 86; no fever; lochia and milk returned.

The two following cases, which occurred in my practice with but a short interval between them, are illustrations of an unusual variety of puerperal fever; the former of the two commencing with diarrhœa, the latter with some symptoms of phlebitis.

CASE XXIV.—DIARRHŒA; SCIATICA.

Mrs. J. L. of S., the wife of a blacksmith, confined Feb. 15, 1851. A very easy, quick, natural, multiparous labour. I was sent for again to this patient, Feb. 21st. I found her suffering from profuse diarrhœa, which had been going on for two or three days. She was feverish; the pulse 100, soft; slight headache; griping pains; milk diminished; lochia unaffected. Sent her chalk-mixture with laudanum. When I called on the 24th, I found her nearly well, with the exception of want of appetite. I saw her no more until May 13th, when I was sent for again,

and found that she was suffering from sciatica, which she stated had commenced within the lying-in month. She was now in a very bad state, weak, hysterical, hectic, pulse 130; complaining of great pain, intermittent, from the hip down one leg, which was colder than natural and somewhat wasted. She said it had been swollen (Query—a slight phlegmasia dolens?) I gave her various medicines with little or no benefit—such as opiates, quinine, colchicum, guaiacum, &c., internally; oleum camphoratum, and other liniments externally, until May 30th, when I commenced the use of galvanism, directing her to have it applied whenever the paroxysm of pain came on. This treatment was followed by perfect success, the pain ceasing on every occasion as soon as the galvanism was felt, and on the 22nd of July she brought the machine home herself.

The following case is introduced here, although unaccompanied by diarrhoea, on account of its resemblance to the last case in its termination:—

CASE XXV.—PUERPERAL FEVER; SCIATICA.

Mrs. V. of Alford, the wife of a groom, was confined of her fourth child, June 25th, 1851. The os uteri was very rigid and thick for several hours; but the application of belladonna ointment at last, along with the administration of a dose of ergot, made it dilate so rapidly, that from a dilatation not larger than a crown piece, very thick and rigid, the labour was completed in half an hour.

The next day she had severe after-pains, with great restlessness, for the relief of which I gave her some opiate doses.

27th—In the evening, she complained of severe pain in the right side of the abdomen. The part was tender to the touch; there were no signs of milk; the lochia scanty; fever; hot skin; headache; white tongue; pulse 115. To take *mist. puerp.*

28th—The bowels have acted well; pulse 110, soft and full; pain still in right iliac region, but it is now *relieved* by pressure; countenance anxious; no lochia, no appetite, no milk. Applied six leeches to painful part of the abdomen, and a warm bran poultice after they dropped off.

29th—Pulse 90, soft; skin cooler, headache less; abdomen tympanitic and rather tender; no lochia; a little milk in breasts; iliac pain somewhat better; bowels well open. Gave calomel and opium to be taken with the mixture.

30th—Pulse 86. Better altogether; countenance no longer anxious; abdomen very full and tympanitic, but less painful, and quite soft; the pain *relieved* by pressure, increasing when the hand is raised; no lochia; no milk. Continue medicine.

July 1st—Better; pulse 80–84; abdomen much less full; tenderness in right iliac region; slight appearance of lochia towards evening; no milk; appetite better; no headache. Continue medicine, with fomentations, &c.

2nd—Greater pain; great restlessness; pulse 86; skin cool; abdomen full and rather tender; no lochia; had only taken two doses of medicine yesterday, none this morning; coldness of right leg; applied eight leeches to right groin. Continue medicine.

3rd—Better altogether, and she continued to improve until the 8th, when pain with tenderness returned in right iliac region; pulse 100, soft; skin moist; tongue clean; lochia abundant; no milk. Applied eight leeches, and recommenced with calomel and opium pills. She was better the next day, but complained much of coldness of right leg. She sent for me again on the 15th. The pain had returned a few days previously, and was now distressingly acute in right hip and groin, striking round to the back; the leg and thigh of that side cold and wasted; pulse unaffected; the milk had never appeared. Affection now purely local, probably neuralgia of origin of sciatic nerve. Gave her veratrine ointment with opium to rub in. On the 17th, the pain being no better, I gave her half a grain of morphia twice a day. This she went on taking, with great relief to the pain, for several days. On the 27th I applied a blister to the right groin. The pain returning a week or two after she left off the morphia, I commenced with galvanism, which entirely removed the affection in about six weeks, by being used as in the last case whenever paroxysms of pain came on.

Why Cases XXIV. and XXV.—especially the latter of them, in the commencement of which there were decided symptoms of phlebitis—did not assume the form of phlegmasia dolens, I am at a loss to understand. In both of them the termination in so purely neuralgic an affection as sciatica was remarkable. In the two cases which will now follow, and in which phlegmasia dolens did develop itself although with fewer premonitory symptoms, the terminations were also somewhat unusual but unfortunate.

CASE XXVI.—INTERMITTENT FEVER; DIARRHŒA; PHLEGMASIA DOLENS; GANGRENE; DEATH.

Mrs. H. of B., near Alford, the wife of a cottager, was confined of her second child, January 5th, 1850. The labour was natural and easy, but there was adhesion of the placenta.

Calling on the fifth day of lying-in, I found her with indefinite uneasy sensations; the pulse about 90; lochia and milk very abundant; bowels relaxed. I was sent for again a week after,

and found she was having rigors every day followed by fever; her pulse when I thus saw her was below 100, but she was just then free from fever. I sent her some doses of quinine to be taken during the intermissions. The next day when I called upon her, she had some fever, and the pulse was about 130. There was no abdominal pain. I ordered the quinine to be continued. A day or two later I found it necessary to give chalk mixture and laudanum on account of diarrhoea. She was now complaining of pain in both legs, which were swollen, white, and pitting (*phlegmasia dolens*). Pulse 120, with some sharpness; no appetite. I gave her oleum camphoratum with opium in it to rub the legs with. *Two days later*—The bowels better; legs worse; pulse 120 as before. *Two days later*—Great complaint made of aphthæ in the mouth; some tendency to diarrhoea; debility great; much emaciation; anxious countenance; legs much swollen—the leg which was the worst now the best. Gave quinine again, and ordered fomentations to the legs.

27th—Frequent vomitings, for which I gave effervescent; other symptoms the same.

31st—Mouth better; vomitings relieved; legs the same; pulse the same; gave dilute sulphuric acid with quinine.

February 3rd—Complaining much of sleeplessness, and of the pain in the legs, which were enormously swollen. Gave morphine with aromatic spirit of ammonia.

4th—Felt better; pulse 115; had slept better; appetite better; legs softer. Continue.

7th—Great pain in legs with gangrenous vesicles on dorsum of left foot, and slighter appearances of commencement of gangrene on right foot; pulse 116. Ordered wine and yeast poultices.

8th—Gangrene much more extensive; vesicles on the middle of left thigh, and on the dorsum of right foot. Pulse 140 and very feeble. Great pain in wrists with swelling, like rheumatism. She died the next morning, it being five weeks on that day since she was confined. The milk was never suppressed throughout, as she was suckling the child two or three days before she died. It was scanty of course. The lochial discharge was also present. The freedom from pain in the abdomen or groin was remarkable, in a case developing itself into such a formidable example of *phlegmasia dolens*. There were, in fact, none of the symptoms of uterine irritation or inflammation which usually precede *phlegmasia dolens*. To be sure, the women in attendance told me she had a swelling in the right groin with pimples on it during the first week, which, however, disappeared in a day or two. The labour had been quick and easy—second child at the full period—third pregnancy; she having miscarried of her

second at the end of the third month. The labour on the present occasion was preceded by a good deal of hæmorrhage which was checked by rupturing the membranes. There was but little difficulty in the extraction of the adherent placenta. During the last week of her illness there was a slough on the back from pressure. When phlegmasia dolens is imminent or is commencing, the application of leeches to the groin or inside of the thighs is recommended, and is frequently efficacious. In the present case they were never admissible. The clearly intermittent character of the premonitory fever called rather for tonics than depletion—as did the diarrhœa; besides all which the presence of the milk and lochia, and the not *very* frequent pulse, deceived me as to the extent of danger; and when the actual disease, phlegmasia dolens, showed itself, there were, *pari passu*, so many signs of failing power as to preclude any kind of depletion. In one word, the absence of abdominal pain was the principal reason why leeches were not thought advisable.

The above remarks stand in my note-book as I wrote them at the time. At the present time I should scarcely have thought them called for. We don't bleed now-a-days. I was at the time passing through the transition state between the system of treating nearly all diseases by depletion, and the present system of stimulation and supporting the strength, which an inexplicable change in the type of disease has gradually forced upon us; and I was endeavouring to explain to myself why I could not then do what I could have done a few years before. We don't bleed now-a-days, because we have gradually found that our patients will not bear it; certainly leeching was not borne well in—

CASE XXVII.—PHLEBITIS; PHLEGMASIA DOLENS; DIARRHŒA;
TYPHOID SYMPTOMS; SUDDEN DEATH.

Mrs. R. P. of B., near Alford, the wife of a miller, and the mother of several children, was confined on *Friday* night, October 25th, 1850. She had an easy natural labour. I saw her on *Saturday*; she was doing quite well, but had a sharp expression of countenance, which made me anxious. On *Sunday* she had severe after-pains, with rigors.

Monday—After-pains severe; pulse small and 120; tenderness in lower part of abdomen; white slimy tongue; lochia sufficient; no milk; appetite bad; countenance sharp. Gave her an ounce of castor oil; ordered fomentations to abdomen, and sent an opiate mixture.

Tuesday—Felt better; pulse 110; appetite still bad; no milk; lochia sufficient; less tenderness. Continue fomentations. The bowels were acting well.

Wednesday—Pulse 112; appetite bad; no milk; no pain complained of in abdomen; less tenderness; bowels acting well; countenance sharp. Continue.

Thursday—Appetite better; pulse 110; some restlessness; no pain in abdomen; tongue rather better; no milk; lochia lessening; complains of pain in the calf of one leg; no swelling nor alteration in appearance of skin; painful part tender to touch. Foment the part.

Friday—Pulse 120; countenance flushed and anxious; skin cool; the affected leg swollen and painful; it is phlegmasia dolens. Applied twelve leeches to groin, and gave three grains of opium and three of calomel in a pill, and an ordinary saline fever mixture. The leg to be rubbed with camphorated oil.

Saturday—There was diarrhoea; other symptoms the same. Sent her a chalk mixture, with laudanum and aromatic confection.

Same evening—Pulse 125, very weak; tongue brown and moist; eyes staring; countenance pale; milk in breasts; no lochia; leg swollen and painful; respiration hurried.

R. Pulv. ipecac. comp.	℥ss.
Ammon. sesquicarb.	℥j.
Spir. æth. nitric.	℥ss.
Liq. ammon. acet.	℥ij.
Mist. camphoræ ad	Oss. M.; Sumat coch. ij;

cum j. pil. seq. quartis horis:—

R. Calomelanos	gr. viij.
Pulveris opii	gr. ij.
Confect. q. s. ut ft. pil. viij.	

Sunday, Nov. 3rd, mune. Pulse 130; more milk in breasts; countenance rather better; respiration not so hurried; not so much pain in leg; reappearance of lochia; tongue creamy-white; complains of pain in right side; there is no cough, and auscultation reveals nothing; applied a blister to side. Continue medicine.

Vespere—Pulse 135; still milk in breasts; countenance anxious; speech dragging; respiration hurried; more pain in right side; calf of leg mottled; inside of thigh, which yesterday had been tender to touch, not quite so painful this evening. Repeat the mixture with double the quantity of ammonia. Continue the pills. An anodyne draught at bed-time. The blister is not acting.

Calling the next morning, I found she had died rather suddenly in the night. The appearance of the milk and the reappearance of the lochia were remarkable phenomena, occurring, as they did, when the disease was assuming a most dangerous character. They are usually regarded as critically favourable.

I learned subsequently, that on Friday afternoon after she had had the leeches, feeling her pain relieved, she got out of bed and called her servant, the nurse not being in the house. The servant not at once answering her call, she stepped out of her bedroom in her night-dress as she was, and with bare feet stood on the landing of the staircase calling for the servant until she came; she then made the servant assist her to get into a cold bed in another room. I suspect that this imprudent conduct led to a sort of metastasis from the leg to the chest. It most unquestionably aggravated the danger of the case; the exertion alone in a case which so rapidly assumed a typhoid character would be dangerous.*

In some cases phlegmasia dolens seems to constitute a sort of critical termination of puerperal fever or irritation, being itself a merely local affection, as in the two following cases:—

CASE XXVIII.—EPHEMERAL FEVER; HYSTERIA; DIARRHŒA; PHLEGMASIA DOLENS.

Mrs. J. G. of C., near Alford, the wife of a village carpenter, of healthy aspect, was confined of her first child, Nov. 22, 1844. Her labour was an average one, rather lingering. The placenta was adherent, and there was considerable hæmorrhage before its extraction. She had two fits of ephemeral fever at the end of the fortnight, during which the pulse was 140. She continued weakly a long time, with a pulse varying from 80 to 90. Toward the end of the month phlegmasia dolens came on, without fever or acceleration of pulse, and not preceded by pain of abdomen, for which I gave her camphorated oil externally and tonics internally. She had been frightened into hysterics three or four days after her first attack of ephemeral fever by some silly women who surrounded her bed and said she would die. In the course of the treatment previous to the access of phlegmasia dolens, she had frequent attacks of diarrhœa and great want of appetite, for which I gave quinine, blue pill, and opium in pills. Mixtures or fluid medicine of any kind made her sick. Her convalescence was protracted, but she had no unpleasant symptoms

* "Death, however, may be caused by suppuration or gangrene, or by exhaustion proceeding from the violence of the constitutional disease; or from exertion made by the patient, which has sometimes proved suddenly fatal."—Burns' "Principles of Midwifery." Fourth edition, p. 437.

And Denman says he had been informed of several cases, and seen one case which proved fatal, in which it appeared on a retrospect that the fatal event might have been averted, "if more regard had been paid to the feelings of the patients, for they were urged to exert themselves beyond their abilities and inclinations, and sunk immediately after, or while they were making some great effort."—"Practice of Midwifery." Sixth edition, p. 463.

after the development of her complaint into phlegmasia dolens. She was of a hysterical constitution.

CASE XXIX.—DISORDER OF STOMACH; FEVER; PHLEGMASIA DOLENS.

Mrs. W. of B., near Alford, the wife of a labourer, an unhealthy-looking woman, was confined June 15, 1859. It was not her first child. Her labour was remarkably easy and quick, but was followed by profuse hæmorrhage *ad deliquium*. She got on very well during the first few days, but I was requested to see her again on the 23rd, Thursday, on which day I have the following note:—

Great agitation; faints when out of bed; complains of headache; forehead hot; was delirious last Sunday, and had a shivering fit last Tuesday; bowels confined; appetite not impaired; lochia still fresh and abundant; tongue clean; pulse 140, fluttered; frequent eructations, and very acid vomitings; her milk is gone, but her child died last Saturday.

Pil. aper. ij. statim sumendæ.

R. Magnesiæ calcinatæ	3j.
Spir. ammon. aromat.	3ss.
Tinct. card. comp.	3ss.
Aquæ	ad Oss. M.
Sumat coch. ij. quartis horis.	

An alterative pill to be taken at bed-time.

Friday, June 24th—Fevered pulse, 140; feels rather better; skin hot; it is reaction without perspiration; abdomen quite free from pain or tenderness, soft, full, and tympanitic; bowels have not been moved; throws up her medicine; her milk has returned.

R. Potassæ bicarb.	3ij.
Tinct. aurantii	3ss.
Spir. æth. nitrici	3ss.
Aquæ	ad Oss. M.
Sumat coch. ij. quartis horis.	

Repeat the aperient pills.

7 p.m.—Better; bowels moved; pulse 120; less fever; less headache.

25th—Pulse 114, a better pulse altogether; no heat of skin; had two rigors yesterday, one in the morning, the other in the evening, the latter followed by perspiration, that is, by satisfactory reaction. Continue.

26th—Better; pulse 108.

27th—Doing well. To take quinine in infusion of roses.

July 1st—Feverish; pulse 120, unsteady; appetite good; milk in breasts; lochia stopping. Continue tonic.

7th—Phlegmasia dolens of right leg; some tenderness in groin, none in abdomen; pulse 120, soft and regular; milk in breasts. Oleum camphoratum to leg; and an opiate pill to be taken.

10th—Pulse 106, pleasant; leg easier; no bad symptoms. Continue camphorated oil.

I have no further notes of this case. The woman made a very satisfactory recovery. The case quoted in the foot-note at page 5, *Journal for April*, is also one in point.

I find that by tracing puerperal diarrhoeas through several varying manifestations, I am gradually drifting into the consideration of yet other varieties of puerperal fever. I have alluded to a change in the type of disease generally, from a sthenic form requiring, and obviously benefited by, active depletion, to an asthenic form in which depletion would be as obviously injurious. As with pneumonia, so it is with puerperal diseases. I was led into the allusion from a retrospect of some of the phenomena in the cases reported. I now propose to present a few cases more directly in illustration—

OF PUERPERAL FEVER REGARDED AS A STHENIC DISEASE.

CASE XXX.—METRITIS.

Mrs. J. C. of C., the wife of a labourer, a healthy woman, was confined of her second child, January 2, 1839. She had an easy labour. I was sent for again on the fifth day of lying-in; I found great tenderness of uterus, which was hard and larger than it ought to have been at that period; parietes of abdomen lax; pain aggravated on turning in bed, or on coughing; pulse 156, small and soft; skin hot, but moist; white tongue; thirst; lochia and milk suppressed. VS. in brachio ad $\zeta xij.$, and to take immediately a powder containing calomel. gr.v. pul v. jalapæ $\mathcal{D}j.$ Half an hour after the bleeding the pulse dropped to 130, and the next day it was only 112; but the day after it had risen to 140, the abdomen still being tender to touch, though the patient expressed herself as feeling better. VS. ad $\zeta x.$ On the following day the pulse was 125, with amelioration of all the other symptoms, and after that she mended without any further medical treatment. I ought to say that after the first bleeding she had saline mixture containing antimony.

CASE XXXI.—PERITONITIS.

Mrs. J. P. of J., the wife of a small farmer, a very healthy-looking woman, was confined of her third child, April 29, 1840,

after an easy natural labour. May 8th, she was aroused from sleep at two in the morning by a sudden seizure of pain. She had a rigor followed by heat of skin and great thirst. There had been considerable lochial discharge since the day when she was confined. When I saw her at half-past seven the pain was very great; countenance anxious; abdomen excessively tender; inability to turn or move; there was no tympanitic distention of abdomen; the pulse 120, small; lochia checked as well as milk; some diaphoresis was present. *VS. ad deliquium* in recumbent position. The pain ceased while the blood was flowing, and the pulse dropped to 100. A dose of castor oil was given, followed by calomel and opium, one grain of the former with a quarter of a grain of the latter every hour. The next day all unpleasant symptoms had left her.

CASE XXXII.—PERITONEAL FEVER.

M. O., an unmarried woman, confined of her second child, March 30, 1841; easy labour. Attacked with abdominal inflammation at the end of nearly a fortnight, in consequence of cold caught. Abdomen tender to touch; pain great and aggravated in paroxysms; countenance anxious; pulse 120, small. *VS.* in brachio. To take a powder containing calomel and jalap; and, after its operation on the bowels, a saline mixture, and a calomel and opium pill with each dose of it. Next day—still feverish; pulse 115; tenderness gone. Continue. The day after—quite better; pulse 85. The blood taken was buffed and cupped.

CASE XXXIII.—PERITONEAL FEVER.

Mrs. B. of J., the wife of a shoemaker, and the subject of the case reported in the foot-note at page 5, which occurred in December, 1837, was confine again March 30, 1840, respecting which latter case I have the following note:—"After-pains severe, with deficient lochial discharge; no acceleration of pulse. Fomentations, with opiates, diaphoretics, and decoct. aloes comp. relieved her and she soon recovered." She was confined again April 12, 1842, after a very severe and lingering labour from rigid os uteri. On the fourth day her lochial discharge ceased. Pains severe; great abdominal tenderness; countenance rather anxious; pulse 80, full. On account of the state of the pulse I trusted to opiates for a day or two, but they gave no relief. I then bled her in the arm with immediate benefit. I then gave aperients with calomel and opium, using frequent terebinthinate fomentations. The day after the bleeding, viz., April 17th, she was better. Continue.

April 19th—Worse; pulse over 100; has had rigors; tenderness excessive. VS. again. Continue calomel and opium with saline mixture containing digitalis.

20th—Symptoms but little alleviated. Applicentur hirudines xij. abdomini. Continue medicine.

21st—Something better; a blister to abdomen. Thenceforward she gradually recovered, but the calomel and opium pills were pushed to ptyalism. Of this same woman's next confinement, in September, 1846, I have the following brief note:—"A very tedious hard labour. Symptoms of peritonitis on the third day rendered bleeding, with calomel and opium, necessary. She was soon well."

The reader will scarcely have failed to notice the great liability to recurrence of inflammatory puerperal fever in the person of the patient whose repeated attacks—four in four successive confinements—I describe above. The case which now follows, besides constituting a fitting introduction to the next group of cases, is also illustrative of this liability in certain women.

OF PUERPERAL FEVER REGARDED AS AN ASTHENIC DISEASE.

CASE XXXIV.—PERITONEAL FEVER.

Mrs. J. P., the subject of Case XXXI., was confined again, July 26th, 1847, after an interval of seven years. The face turning to the pubis, the labour was perhaps not quite so easy as on the previous occasion. She was attacked on the ninth day with symptoms similar to those described as following the previous labour, except that she had no decided rigor. There was also less heat of skin, and less anxiety of countenance; the pulse 120, soft; diaphoresis present when I saw her. She recovered without bleeding this time, using fomentations, and taking a few doses of calomel and opium with salines.

CASE XXXV.—PERITONEAL FEVER; METRITIS; DIARRHŒA.

Mrs. W. K. of Alford, the wife of a labourer, a delicate woman, was confined after a tedious labour, with rigid os uteri—multiparous—June 2nd, 1850. Calling on the fourth day of lying-in, in the evening, June 5th, I found her just rallying from a shaking fit, of which she said she had had three. She was feverish and restless; the pulse 150, small. There was abdominal pain and tenderness; body of uterus hard and tender; tongue white; lochia sufficient; milk gone, she having been using spir. camph. to her breasts in consequence of the death of her child which was small, puny, and premature. The bowels had

been moved, but were now confined. I gave her a calomel aperient, and a mixture with Dover's powder and camphor julep, ordering fomentations. The next morning, she felt better; pulse 120; some perspiration. Continue medicine and fomentations. In the evening of the 8th the pulse was below 100. On the 10th I was summoned at 5 a.m. I found her suffering from diarrhoea; the pulse 108; some perspiration; less pain in abdomen, which was, however, full and tympanitic. Gave chalk mixture with laudanum. She now gradually recovered, the pulse seldom after this getting above 100.

CASE XXXVI.—PERITONITIS.

Mrs. W. of W., near Alford. During my temporary absence from home, my assistant was sent for by a midwife to lend his aid in the delivery of this patient, at 6 a.m., November 27th, 1850. She was the mother of a large family, and the midwife had been in attendance about forty-eight hours. She had ruptured the membranes the day before, was unable to feel the presentation, and there was no pain. Pains had gradually come on, which had increased until an arm came down. Turning, doubtless, would have been easy enough when the membranes were first ruptured, but Mr. S., my assistant, although he had given her two full doses of laudanum, found it impossible to get his hand up. He desisted after a quarter of an hour's efforts, and sent for Mr. C. of W., a distance of about a mile. Mr. C. tried perseveringly and patiently for two hours, and was about to desist from what appeared to be a useless attempt to pass his hand into the uterus, when all at once, a gush of blood came away. The uterus relaxed, he passed up his hand, turned and delivered. The child was dead. This was about noon. Mr. S. saw the woman again in the evening, and found her very bad. The next morning, having returned home, I saw her myself; the pulse 120, soft and reasonably full; great abdominal tenderness with tympanitic distention; no lochia; tongue white; some nausea; she had not made water. I passed the catheter and drew off a large quantity of urine, which gave some relief to her pain. A dozen leeches were applied to the abdomen, and she was ordered to take a mixture containing *magnesiæ sulph.* ℥j. in eight doses, each of which was to be taken with a pill containing two grains of calomel and half a grain of opium.

29th—Pulse 120, weaker; great tympanitis; great flatulence of stomach; lochia sufficient; bowels confined. Gave an enema, which brought away a very offensive motion. Continue medicine.

30th—Pulse 115, stronger and sharper; lochia abundant; less

pain; some tenderness on pressure; abdomen softer, but very full; great thirst; bowels open. Continue medicine.

December 1st—Found her sinking. She died the next morning at 10.

Post mortem examination in the afternoon of same day, with the assistance of Mr. C. and Mr. S.—Uterus large; peritoneum gangrenous; slight adhesion of intestines to uterus, easily broken up. Internal surface of uterus showing a black gangrenous patch on right side, probably the part bruised in the attempts at turning. There was no rupture.

The two following cases are specimens of an adynamic variety of puerperal fever now frequently met with in practice:—

CASE XXXVII.—PERITONEAL FEVER.

Mrs. B. of A., near Alford, confined of her first child, February 2nd, 1850. Sharp labour, with rigid os uteri. Calling a week after, I found her feverish, with headache, and a pulse 160, soft. No abdominal tenderness; bowels confined; no appetite; tongue white; lochia and milk sufficient; had had rigors. *Mist puerp.* Ordered also a dose of castor oil. Next day she was better; pulse below 100, and she was soon all right. But ten days after, I was sent for again. Fever; pulse 120, sharp; complaining of pain above pubis, with some tenderness; headache; appetite bad; to take *mist. puerp.* again. Next day the pulse was 80, and I heard no more of the case.

CASE XXXVIII.—PERITONEAL FEVER.

F. P., an unmarried woman, multiparous, confined after an easy labour, August 18th, 1850. After-pains severe for two or three days; better after an opiate. I was hastily sent for in the night of the 29th August, the 12th day of lying-in. She had had a shivering fit, followed by fever; pulse 110, rather small and sharp; abdomen painful and tender to touch; great pain in head; some anxiety of countenance; skin moist; bowels confined; tongue white; lochia scanty; milk in breasts. To foment and take eight grains of calomel, and afterwards go on with *mist. puerp.* The next day all the symptoms were better; the pulse 100. The day after she was quite well. The child having refused the breast, when the woman was taken ill, a woman sucked the milk and found it tasted *salt*.

The following, also an adynamic case, is a specimen of a more troublesome form of puerperal fever not unfrequently met with since the change in the type of disease which I am now endeavouring to illustrate:—

CASE XXXIX.—ACUTE TYMPANITIS; DIARRHŒA.

At 10.30 p.m., February, 4th, 1854, Mrs. R. of Alford was suddenly surprised by an immense gush of blood. She fainted, and was carried up stairs to bed. As I was detained at the time in attendance on another case at a considerable distance from home, I did not see her till 1.30 a.m.; meantime she was under the care of one of my professional neighbours. On my arrival, I found the cranium presenting in the first position; the os uteri dilated to the size of a shilling, rigid, and rather thick. There were very slight pains at considerable intervals, each accompanied by a gush of slightly-coloured serous fluid. I immediately ruptured the membranes, after which the flow of fluids ceased entirely. There was, however, some faintness felt occasionally; there was frequent yawning, and a very blanched countenance. I then gave a full dose of ergot. This speedily brought on nice pains, and the labour was safely completed at 4.15 a.m. The child was quite dead, probably in consequence of the hæmorrhage, as it had been felt to move just before the labour began. Immediately after the child was born, a very large firm coagulum, larger than the after-birth, was expelled. I think it probable that the gushes of pale fluid which took place with the first slight pains were composed chiefly of the serum of the blood, the coagulum of which was expelled after the birth of the child. The opening through the membranes was found to be close to the edge of the placenta. I could not feel any portion of the placenta during the labour. A considerable portion of the maternal surface of the after-birth, near the opening through the membranes, was found to be covered with a thin layer of very adherent and firmly coagulated blood.

During the remainder of that day, Mrs. R. felt exceedingly well, the pulse and appetite being perfectly good. The next day the pulse was quick, but there was no other unfavourable symptom. On the third day, in the morning, I found her suffering from great pain in the abdomen, with tympanitis, and great tenderness on pressure. The pulse was 140, weak; tongue clean; no shiverings; lochia and milk all right. There were frequent eructations. I ordered fomentations. She had some difficulty in micturition. I gave her—

R. Liq. morph. acet. (Ph. Lond.)	℥ss.
Spiritus ætheris nitrici	℥j.
Aquæ ad	℥iiss.

M. Fiat haustus statim sumendus.

A calomel and compound scammony powder was ordered to

be taken an hour after, the bowels not having been moved since the confinement; and she was directed to go on with the following mixture—

R. Pulv. ipecac. comp.	℥ss.
Magnesiæ sulphatis	℥vj.
Spiritus ætheris nitric.	℥ss.
Aquæ menthæ piper. ad	℥vj.

M. Fiat mistura cujus sumantur cochlearia ij. tertiis horis.

In the evening she felt better; she had less pain and tenderness; could pass urine; the bowels had acted once slightly; pulse 120. There had been no headache to-day, though some was felt last night.

Fourth day a.m. Pulse 125, very feeble and indistinct; no pain; tenderness and swelling nearly gone. She was flatulent, and had been sick several times. The bowels had acted three or four times in the course of the night. She complained of thirst, and was *very smiling*.

R. Confectionis aromaticæ	℥iss.
Tincturæ opii	℥iss.
Spiritus ammoniæ aromatici	℥ij.
Magnesiæ calcinatæ	℥j.
Aquæ menthæ piperitæ ad	℥vj. M.

Sumat cochlearia ij. tertiis horis.

Pulse at noon, 125, fuller; at night, 140, *fluttered*. The bowels were acting too much. The mixture was continued.

Next day (fifth) the pulse was feeble and palpitating, 125–130. She had had a good night; was not sick; had no appetite; tongue clean; forehead hot. The bowels had acted again very freely after I paid my visit last night.

R. Cretæ præparatæ	℥ss.
Confectionis aromaticæ	℥iss.
Ammoniæ sesquicarbonatis	℥ss.
Tincturæ opii	℥ij.
Aquæ menthæ piperitæ ad	℥vij.

Sumat cochlearia ij. quartis horis.

4 *p.m.*—She was better; pulse steadier, 120. The bowels were quieter.

Next day (sixth) *a.m.*, pulse 100; *p.m.*, 90. Her appetite was returning; bowels quiet; and so she recovered rapidly.

CASE XL.—PERITONITIS; MALIGNANT PUERPERAL FEVER,

Mrs. H. B. of S., near Spilsby, the wife of a farmer, primipara, was confined early in the morning of January 30, 1852. A

rather severe labour; ergot given; head an hour in pelvis after full dilatation of os uteri, and another hour on perineum. Child living. I remained with her two hours; after-pains very severe. Gave a dose of laudanum and ordered fomentations. Called the next day and found she had had no pain since using the fomentations; she felt quite well; was very cheerful; appetite good; abdomen soft and quite free from tenderness—but the pulse was 140! I ordered a dose of castor oil; and left instructions with the nurse to send for me immediately if she should have a shivering fit, for I did not like the pulse at all. I was sent for the following afternoon. She had had two rigors the night before—one at nine, the other at ten—and had had great pain in the abdomen ever since. I found her suffering from excruciating pain all over the abdomen, aggravated in paroxysms; pain also in loins; abdomen excessively tender to touch; she was lying on her back with her knees up, unable to bear the bed-clothes; no distension; the lochia, which had been abundant, now checked; no milk; countenance anxious, that is, expressive of suffering, but she can smile; pain in forehead, with dizziness; tongue clean; no appetite; great flatulence of stomach, with continual eructations; the bowels moved twice last night; there was no great heat of skin, and she complained much of thirst; the pulse was 145, soft and weak. *VS. in brachio statim ad deliquium*, in recumbent position; bleeding borne well, two pounds being drawn before she fainted. *Sumat statim calomelanos gr. x.* To foment the abdomen, and take *mist. puerp.* and a grain of calomel with each dose every four hours.

February 2nd, 9 a.m.—Is better; skin cool; pulse still 145, softer; less pain; no paroxysms; abdomen still tender to touch; is lying on her side. I was told that an hour after the bleeding yesterday, she turned on her side and slept three hours. Slept well last night; no pain in head, but is still rather dizzy. Countenance better and more cheerful; a flush in one cheek; bowels relieved once slightly yesterday. Tongue slightly coated with moist dirty-yellow fur; still thirsty; respiration easy, as, indeed, it was yesterday; not so flatulent; lochia watery; no signs of milk; blood drawn very buffy. *Applicentur hirudines xij. abdomini.* *Cont. fetus et med.*

7 p.m., same day—Bowels have not acted again; great restlessness, with flatulence and dizziness; pulse 150; abdomen not so tender. An enema of gruel and turpentine brought away an immense accumulation of scybala, fæces, and flatus, giving great relief. Lochia a better colour. Continue medicine.

3rd—Is much better; pulse 120, firm; slight fever; no pain; tenderness better, but still felt, chiefly towards left groin. Bowels relieved twice more, with more scybala. Has

been bleeding at the nose. Slept well last night; is now very cheerful and smiling; a little flatulence; tongue the same; lochia the same; no milk. Continue medicine, without the sulphate of magnesia. To foment with turpentine sprinkled on the flannels.

4th, 11 a.m.—Not so well; pulse 130, soft; tendency to paroxysmal pains again, during which the abdomen becomes hard and more tender to touch; in the intervals the abdomen but slightly tender; what tenderness there is, is chiefly in left groin. Head feels flighty. Was delirious in the night and did not sleep well. She is talkative and cheerful; tongue the same; mouth feels dry, and she complains of her throat feeling sore in swallowing; slight appearance of lochia; no milk. Bowels have not acted again. *Applicentur hirudines vj. inguini sinistro. Cont. med. et fofus tereb.*

At midnight—Much worse; pulse 140; great pain, distention, and tenderness. Difficulty and pain in micturition, so that she has refrained too long. Can still turn in bed, but with difficulty; no headache; no delirium; lochia checked; tongue brown and dry; great thirst. Drew off about a pint of urine with catheter. Gave a turpentine enema. Ordered turpentine stupes to be continued; gave her a grain of morphia in a draught. Stayed till 2 a.m., and left her disposed to sleep.

5th, 9 a.m.—Found her sinking. She died at 10.

At the time, I was much annoyed at not being sent for until so many hours after the first rigor,* for I thought with pain and regret on what I quote in the foot-note below. I think now, however, that as Blundell was writing when these diseases were much more frequently sthenic in type than they have been since the year 1847 or thereabouts, when, in short, depletion was in its hey-day of success, his remarks are scarcely applicable to such a case as the one just reported, occurring as it did in 1852. The following example, however, shows that we may still occasionally meet with puerperal fevers in which bleeding may be beneficial. Although somewhat out of place in the present group of asthenic cases, I introduce it here as an exceptional case. It is instructive *because* it is exceptional. It is interesting, also, as having occurred so late in the lying-in month, as the 27th day.

* "In using venesection . . . it is of the greatest importance to commence the bleeding as early as may be. I have laid it down as a sort of rule in my own practice, that if, in the less vehement attacks, the bleeding be commenced within six hours after the chill, your patient will be saved often, and if within twelve hours, not unfrequently; but that if you do not begin until twenty-four hours have passed away . . . the patient will usually die."—*"Blundell on Puerperal Fever."* Lancet, August 23, 1828, vol. xiv. p. 643.

CASE XLI.—PERITONITIS.

Mrs. R. W. of Alford, the wife of a blacksmith, aged only 17, was confined, after a remarkably easy labour, of her first child, November 2, 1853.

On Monday, November 28th, the 27th of lying-in, I was requested to call. I found her sitting up and suckling her child. She was complaining of pain in the abdomen. She said she had had a shivering the previous Friday. The bowels were open. The pulse below 100. To take *mist. puerp.* and foment.

November 29th, a.m.—Great pain in abdomen, great tenderness over the whole of it; white angry-looking tongue; pulse 150; countenance flushed; respiration hurried. *Applicentur hirudines vj. abdomini*, and foment. To take two strong aperient pills containing calomel, and continue mixture.

Same day, p.m.—Pains relieved, but tenderness as great and extensive as ever. It is thirty-six hours since she passed any water; used the catheter. *V.S. in brachio ad ℥xij.* Continue mixture, taking a grain of calomel with each dose.

30th—Feels better, but the pulse is 156; blood drawn very buffy; tympanitic distension of abdomen. Foment with turpentine. Continue medicine.

December 1st, a.m.—Pulse 140; respiration hurried; tongue still white on top, but not so red at tip and edges.

Same day, p.m.—Pulse 125; *App. empl. canth. abdomini*.

2nd, a.m.—Has had a good night; feels much better; pulse 120; abdomen still tympanitic, but not so tender. Continue medicine.

Evening—Very drowsy, has been so all day; pulse still 120. I don't like this drowsiness. Continue.

3rd, a.m.—Pulse still 120; with tympany and some tenderness of abdomen; otherwise feels better; drowsiness gone. Continue medicine.

4th—Pulse 114; some return of appetite.

5th—Pulse 108; is gradually leaving off her medicine.

6th—Pulse 100.

7th—Pulse 90; and so she recovered steadily.

It was with much hesitation that I bled this patient; probably if she had *not* been bled she would have died, and, as probably, if the subject of Case XXXIX. *had* been bled, she would have died. But I cannot understand why the last reported case should have recovered, while Case XL. died.

I now pass on to the consideration of a subject of great interest; the cases to be reported in illustration occurring in the sthenic period, viz., in 1842.

OF PUERPERAL FEVER REGARDED AS A CONTAGIOUS DISEASE.

The following cases, which are corroborative of the opinion that puerperal fever may originate from the attendance on erysipelatos patients, and that a malady so originating is communicable from one puerperal woman to another, are also suggestive of other conclusions on this painful subject, which are, I think, of some practical importance.

On the 6th of January, 1842, I was called in to attend an elderly gentleman in a village where I was then residing, who had erysipelas of the face. As yet his symptoms were not at all urgent; it appeared to be quite a slight and common case. I sent some saline medicine, and a lotion. The same evening I attended a case of midwifery, two miles from my residence. The next morning, before visiting my erysipelatos patient, I attended a second case of midwifery, four miles from my residence. On my return home I found my patient considerably worse; I applied nitrate of silver freely all over the affected part. At one or two o'clock in the morning of the next day, January 8th, I was called up to him. He was much worse; face and head frightfully swollen down to the shoulders on both sides. After visiting him again the same morning at nine o'clock, I called upon both my lying-in patients. They were doing well; and in fact, recovered without a single bad symptom. In the evening I found the old gentleman in a very bad way; there was some delirium, and the head and face were still more swollen. I made a few punctures in both cheeks, and while busy with him, was sent for in all haste to attend a case of midwifery, Mrs. T. T. of S., a distance of three miles. I believe I went without washing my hands. I was not detained more than a couple of hours, and as I was riding homewards I was met by my groom, who had been sent to me with the announcement that an alarming change had taken place with my erysipelatos patient. I hastened on, and found him comatose, with a dusky livid appearance of the face. He died in the course of the night, a frightful spectacle.

CASE XLII.—PERITONEAL FEVER; DYSURIA.

And now commenced my troubles. In the night of the 9th January, or rather early in the morning of the 10th, I was again summoned to Mrs. T. T. I found her in the following state:—Very severe pain, aggravated in paroxysms, like after-pains; some abdominal tenderness; had had a shivering fit; some headache; no anxiety of countenance; pulse 120, soft and full; skin hot; diaphoresis; lochia abundant; no signs of milk, but as

it was barely thirty hours after delivery, it was quite early enough for that. I ordered fomentations, and sent some doses of Dover's powder in a saline mixture. Eight hours after, pulse 100; felt much better. Sent her an opening powder—calomel and rhubarb.

January 11th—Pulse 112; more pain again, with tenderness; *complained much of pain and heat of the vulva*. Ordered eight leeches to the abdomen, and four to the vulva, and gave her a grain of calomel and three grains of Dover's powder with saline mixture every four hours.

12th—Tenderness increased; abdomen tympanitic; pulse 130, smaller, but not sharp; some anxiety; lochia still abundant; no milk; tongue white and slimy. I proposed venesection, but finding the husband inclined to be dissatisfied, I made him fetch my friend Dr. Barker of Spilsby, with whose sanction I bled the patient. The medicines were continued.

13th—Pulse below 100; pain better; abdomen very full; did not pass sufficient water.

She continued to improve till January 16th, though very nervous and timid, and on that account giving a good deal of trouble; but in the evening of that day I found it necessary to pass the catheter, which had to be repeated once or twice a day until the 26th, when the retention, after resisting various medicines, such as tinctura lyttæ, strychnine, mucilage with liquor potassæ, &c., yielded at once to the tinctura ferri muriatis, in doses of fifteen drops every four hours. The pulse never got above 100 after the venesection. She continued weakly for about a month.

On the 15th of January, at one o'clock in the morning, I attended a case of midwifery in a village about six miles from my residence. The woman had "got it over" before my arrival; I had not even occasion to remove the placenta. I, however, felt her pulse, of course, besides ascertaining by pressing the abdomen that the uterus was safely contracted, which I never omit to do before leaving a lying-in woman.

On the 17th I called upon this patient again, after having in the morning passed the catheter for Mrs. T. T. She was getting on well, and *recovered without the occurrence of a single unpleasant symptom*.

CASE XLIII.—PERITONITIS; MALIGNANT PUERPERAL FEVER.

In the evening of the 16th of January, immediately after my return home from a visit to Mrs. T. T., on which occasion, be it remembered, I had been obliged to use the catheter, I was summoned to attend Mrs. R. B. of C., about a mile from my

residence. I was with her about an hour before she was delivered; it was her fourth child, and her labour was easier and quicker than common.

On the 18th in the morning, being about thirty-six hours after her delivery, I was again summoned to this patient. She was suffering very severely from constant pain in the abdomen, aggravated in paroxysms (which I have generally found to be the case in *puerperal* peritonitis); there was very considerable tenderness on pressure; pain in the forehead; pulse 130, small and sharp; respiration hurried; countenance flushed and anxious; restlessness; lochial discharge slight; bowels confined; some nausea; tongue white; skin hot; could not find that she had had any rigors. I bled her in the arm immediately, after which the pulse dropped to 100, and the pain was somewhat abated. Sent an opening powder, and some fever medicine.

When I called in the evening the aperient had not acted; the pain had returned as bad as ever, the tenderness being excessive; pulse 130, as at first. I took some more blood from the arm, and gave her an enema, which brought away some scybala. The pains were now materially relieved, though the pulse did not drop this time. Sent an opening mixture, with magn. sulph. and tinct. hyoscyami.

19th—Felt better; purged two or three times; no return of paroxysmal pains; tenderness still remaining; pulse 130. The milk had appeared in the breasts, which I looked upon as favourable.

20th—Complete retention of urine, causing pain and tenderness again. Passed the catheter, which gave great relief. In other respects better.

21st—No water passed since the catheter was used at noon the day before, catheterism therefore necessary both morning and evening; there was still *malaise*, though the symptoms were apparently less urgent; there was still pain, with abdominal tenderness; and the pulse was never below 130. I therefore this day gave calomel and opium in pills, and applied a dozen leeches to the abdomen.

The next morning, January 22nd, the catheter was not needed; there was some tendency to diarrhoea, and she had a severe rigor. I found all the symptoms aggravated. Any further depletion I could not venture upon; on the contrary, I sent some aromatic confection, continuing the calomel.

I saw her twice or three times on this day. I was called up the next night, and finding the result likely to be fatal, persuaded the husband to fetch Dr. Barker. By the time he arrived, in the course of the morning of the 23rd, the poor woman was beginning to be typhoid; she had been at times delirious; there

was tympanitic distention of the abdomen, with other untoward symptoms. We gave carbonate of ammonia, to which was added wine the next day.

On the 25th she was quite typhoid; black tongue; subsultus tendinum distressingly frequent; hiccup, &c.; and in the evening of the same day she sank. A constipated state of the bowels in the first instance seemed to complicate the disease unfavourably. There were some scybala voided even on the 23rd. The milk continued in the breasts to the last; its appearance, therefore, in this disease, is not critical, as indeed I have had occasion to remark in some of the previous reports.

Dr. Barker and I discussed the matter most anxiously while he was with me on the 23rd, and although we were both of us quite unconscious that the case of erysipelas had had anything to do with the puerperal affection, we felt compelled to the decision that Mrs. R. B.'s case was genuine malignant puerperal fever, and that as it had probably been communicated from Mrs. T. T., my only safe course to prevent its further spread would be to discontinue attending midwifery for a time. I therefore at once sent for a substitute. But before his arrival (I had twenty-three miles to send) I was *obliged* to attend another midwifery case. I was terribly anxious. I changed all my clothes before going; washed my hands in several waters, with solution of chloride of lime; and although I delivered the woman, I was careful to make as few examinations as possible. This case, it will be understood, was attended on the 23rd of January. The woman had some slight symptoms of peritonitis on the 25th; her pulse was very frequent, and there was severe pain with some tenderness. She had some saline aperient medicine, with fomentations, and did well.

As soon as I could be released from my attendance on Mrs. T. T.—for I did not think it prudent to allow my substitute to pass the catheter for her, or even to see her—I went on a visit into Leicestershire for a fortnight. I am happy to say that I found, on my return to my practice, that I was completely disinfected.

I may be allowed to observe upon the above accurate detail of facts, that if it be conceded that my attendance on the case of erysipelas was the cause of this formidable malady getting into my practice, which, painful as the thought may be, I think was the case, it seems probable—

First, That as the cases attended on the 6th and 7th of January, recovered without the appearance of any unfavourable symptoms, a form of erysipelas, which may subsequently in its *malignant* and almost *putrid* stage originate puerperal fever, will not do so in its *early* stage.

Secondly, That as the disease did not come on in the same patients, notwithstanding my visiting them after attending the erysipelatous case when the symptoms *had* become formidable, the merely taking a lying-in woman by the hand will not originate the disease.

Thirdly, That as the woman attended on the 15th of January recovered without any symptoms of the malady, although I might at that time, be supposed to be tainted with both the erysipelas and the puerperal disease of Mrs. T. T., it requires something more than a mere *visit* to a parturient woman to communicate the disease from one puerperal patient to another.*

Fourthly, That, bearing in mind the above suppositions, the circumstances immediately preceding and accompanying the attendances on both Mrs. T. T. and Mrs. R. B., go to prove that it requires actual contact of the accoucheur's infected hand with the mucous membrane of the vagina, both to originate and communicate the disease. (Query—would not the heat and pain of the vulva in the first case confirm or strengthen this supposition?)

Fifthly, That the slight manner in which the disease appeared in the woman attended on the 23rd (notwithstanding I was still passing the catheter daily for Mrs. T. T., and had but just returned from a visit to Mrs. R. B., the latter being at the time almost typhoid), would seem to prove that ablution, change of clothes, chlorides, &c., are of *some* little avail.

Sixthly, That erysipelas may originate a *mild* form of puerperal fever, which may in its turn communicate a *more malignant* form. (In the two cases here alluded to, the symptoms in the commencement were nearly the same, the occurrence of dysuria in them both being somewhat remarkable; the fatal difference was in their *terminations*—typhus and death in the one, gradual recovery in the other.)

And *lastly*, that a puerperal fever, originating from a case of erysipelas, is not *necessarily fatal*.

I offer the above remarks with considerable diffidence. The *facts* I have detailed may be relied on, and I leave the reader to found upon them what conclusions he may please.

(*To be continued.*)

*From what Dr. Blundell says in his lectures (*vide Lancet* for August 30th, 1828, page 677), he appears to think a mere visit dangerous.

II.—*On Spinal Irritation*. By JOHN PAXTON, M.D., &c.,
Kilmarnock.

(Read before the Glasgow Medico-Chirurgical Society.)

IF the question were to be asked why I have chosen spinal irritation as the subject of these few notes which I now have the honour to read to you, the answer would be, that the disease is of considerable importance to the practitioner, and one the diagnosis and proper understanding of which may often throw unexpected light on most puzzling and apparently anomalous cases.

I do not know whether it now generally meets with the attention it deserves, particularly from that part of the profession who have the indubitable advantage of being considerably our juniors; but I know that "in our hot youth when the fourth George was king," we used to meet with cases in all the journals pointing out its symptoms, and illustrating the proper treatment. It is, therefore, to refresh the recollections of my contemporaries, and to call the attention of our juniors, if it should be needed, to this class of affections that I am induced to collect these few remarks, and to look into my notes for one or two cases which I thought interesting at the time they occurred. Not to waste your time I shall as briefly as possible proceed to give a sketch of the symptoms and treatment of this disease, and finally, shall detail two or three cases which I think illustrate the necessity of keeping the probability of this origin of symptoms in view, for the successful treatment of a class of cases with which we are constantly meeting.

Dr. Copland defines "spinal irritation, or rachialgia, to be—Pain in some part of the spinal column, generally accompanied by neuralgic or hysterical affections, unattended by fever or by other indications of inflammation, injury, or structural change of the vertebral column or of its contents."

Among the writers on this subject, the late Mr. Brown of Lanfine stands conspicuously forward as one of the first to call attention to the frequency of this ailment, and to the danger of its being overlooked; and I may mention, "par parenthèse," that that gentleman, after occupying for many years the enviable position of a highly successful physician in this city, lived for many years more to enjoy the still more enviable position of a dignified country gentleman, respected by all in his neighbourhood, residing on his paternal acres, and having ample leisure to cultivate his literary and scientific tastes. About the same

time, or after him, came Teale, Darwall, Tate, the Griffins, &c., all having cases and observations more or less valuable, and throwing light more or less clear on the subject.

To enumerate all the symptoms of spinal irritation would be to enumerate the various diseases which flesh is heir to, as they have all been simulated by the disease in question; they are chiefly, however, located in that portion of the body which receives its supply of nerves from the segment of the spinal marrow occupying the irritable portion of the spine, and have a general character corresponding to the manifestations of morbid action in the organs of the different regions.

When the cervical portion of the spine, for instance, is the seat of tenderness, we may have painful affections of the head, aphonia, difficult deglutition or respiration, hesitation, and difficulty of speech. When the dorsal region is affected we may have spasmodic actions of the arms, pains about the chest or in the side, or a feeling of weight or constriction, cough, vomiting, gastrodynia, pain in the region of the liver, &c. And when the lumbar is the seat of the irritation, we have pains in the abdomen, dysury, pains in the testes, and perhaps partial paralysis of the lower extremities. As a general rule almost all of these symptoms, and the pain of the spine itself, are much increased by pressure applied to the irritable portion of the vertebral column.

Women of a nervous temperament are very frequently the subjects of spinal irritation; it is also frequently complicated with hysteria, although they are not necessarily concomitants, and are by no means convertible terms, as one might be led to infer on seeing the similarity of the phenomena which accompany either disease.

The disease is very variable in its duration; sometimes yielding in the most gratifying manner to appropriate treatment, and at others resisting every curative means which can be devised.

As the symptoms are often so violent as to be readily mistaken for organic disease of the vertebræ or spinal marrow, on the one hand, or for inflammatory or other actual affections of the organs or tissues which are the seat of pain on the other, it is of the greatest importance to be able to distinguish between the real and the simulated diseases. To enable us to do this, the Messrs. Griffin in their excellent works have given us the following diagnostic rules, which for clearness and accuracy cannot, I think, be surpassed, and which I take the liberty of quoting:—

1st. The pain or disorder of any particular organ being altogether out of proportion to the constitutional disturbance.

2d. The complaints, whatever they may be, usually relieved by the recumbent posture, always increased by lifting weights, bending, stooping, or twisting the spine, and among the poorer

classes, often consequent to the labour of carrying heavy loads, as in drawing water, &c.

3rd. The existence of tenderness at that part of the spine which corresponds with the disordered organ, and we would add the increase of pain in the organ by pressure on the corresponding region of the spine.

4th. The disposition to sudden transference of the diseased action from one organ or part to another, or the occurrence of hysterical symptoms in affections apparently acute.

5th. The occurrence of fits of yawning or sneezing, which though not very common symptoms, yet, as scarcely ever occurring in acute or organic diseases, may generally be considered as characteristic of nervous irritation.

The nature of spinal irritation has been the subject of much discussion. Into this I do not think it necessary to enter, but may state in so many words that the balance of opinion seems to be in favour of regarding it as connected with, if not the result of, congestion of the spinal circulation.

The treatment of this disease is I believe not difficult, though not always successful.

When it is suspected as being the cause of the morbid symptoms present in any case, the spine is carefully examined by pressure and percussion of the vertebræ. Should tenderness be detected, leeching or cupping are generally found of service, and may be advantageously followed by counter-irritation in its various forms, as blistering, seton, frictions with croton oil or tartar emetic, &c.; rest is also almost essential; change of air and scene, sea-bathing, steel, and other tonics, &c., are all useful. I need hardly add that, in this as in every other disease, much success in treatment cannot be looked for, unless the state of the general health is carefully attended to, in addition to any local or special remedial measures which may be adopted.

I shall now shortly mention one or two cases which I think illustrate in some degree the remarks I have had the honour of reading to you.

Case I.—A boy, 14 years of age, was roughly handled by his comrades, and finally thrown down in such a way that the upper portion of the dorsal and lower of the cervical vertebræ came into pretty severe contact with the ground. Some time afterwards it was found that he could neither of himself raise, nor allow his left arm to be raised from his side, not from paralysis, because the motion of the forearm and hand were unimpaired, but from a condition of tonic contraction of the flexors of the shoulder-joint. He made no complaint of pain of the back, and his general health was pretty good. This lad was seen by more than one practitioner and bone-setter. Opinions as to the nature of the case

were various, and a good deal of treatment had been applied to the shoulder-joint without success. On seeing the case, it occurred to me to make an examination of the spine; I found great tenderness to pressure of the lower cervical and upper dorsal vertebræ, and by three or four successive cuppings at intervals of several days, had the satisfaction of restoring to the patient the complete use of his arm.

Case II.—A boy, 12 years of age, after exposure to cold and wet, became the subject of complete aphonia. He could articulate perfectly well, but only in a whisper; was quite active and hearty; complained of no pain anywhere; and nothing morbid could be detected in the throat, so far as it could be seen. He also had had advice from several practitioners, had used all sorts of gargles and tinctures, and had leeches and blisters applied to the front of the throat over the trachea, without beneficial effect. As the aphonia was of many weeks' standing, the boy's parents were naturally anxious. On seeing him I detected, after examination, decided tenderness in the upper cervical vertebræ, and had him leeches several times in that situation without improvement. He was then blistered, the blister being applied at bedtime. In the morning his mother was surprised, and even a little alarmed, at his shouting from his bed, "Mother, is it time to go to school yet?"

Case III.—A friend from Liverpool related a case to me the other day where he had been called in in consultation, and found the patient being energetically treated by bleeding, &c., &c., for acute peritonitis. He doubted the reality of the peritonitis, turned his attention to the spine—discovered two very tender points, and had the satisfaction, by the application of blisters to these points, kept open for a few days, of restoring the patient to health. It took a considerable time, however, to remove the anæmic condition induced by the copious bleeding.

Case IV.—A young girl between 16 and 17 years of age, the daughter of a small laird, became subject to various anomalous painful and hysterical symptoms. After a time she was seen by a medical gentleman home from India, with the prestige of an Indian reputation. He detected spinal tenderness, and formed the opinion that it was a case not of spinal irritation, but of organic disease of the spine, and amongst other things recommended the girl to recline on a board. This was so faithfully done that at the end of nearly a couple of years I found her still reclining on her board. The hysterical symptoms, and the accompanying self-will and self-indulgent habits, had in the meantime increased so much that she could not be induced to leave her room or even her board; she could not bear light, noise, fresh air, or even that any one, unless in the most stealthy manner,

should enter her room. The windows and shutters were kept closed, the crevices pasted up with slips of paper, and she lay on the same blankets till they rotted beneath her, when new ones were with difficulty spread under her. The room was fetid, and the walls damp and clammy from condensed perspiration and the vapour exhaled from her lungs. After a careful examination of the spine, I could detect no organic disease, but at more than one point great tenderness to pressure. She was recommended to be taken out, washed, cleaned, and put into an airy room; to have steel and other tonics, with counter-irritation to the tender points, &c., &c. The parents, however, although very anxious for her welfare, were too much under her control, and too infirm of purpose, to have any rational plan of treatment enforced. The end was that she became phthisical, and died in a year or two after I saw her, never having left the filthy den in which I found her. Probably had a different plan of treatment been adopted from the first, the result might have been much more satisfactory and creditable to all concerned.

These cases I consider peculiar, and, with the exception of the last, fortunate in their issue. We cannot, however, always expect to have our treatment followed by such immediate and successful results. In a less remarkable, though not less important form, we meet with similar cases almost daily; and I should be perfectly satisfied if I could think that these hasty and imperfect notes may have the effect of turning any one's attention more to this subject, and of giving a proper direction to the treatment of cases, which unless diagnosed, are by far the most annoying and unsatisfactory, both to patient and practitioner, that can be met with.

III.—*On the Laws of the Mutual Interpenetration of Fluids.*

By JOHN MURRAY, M.A., M.D.

No. II.

IN my former paper on this subject I treated of the laws according to which those fluids which under ordinary circumstances are sensibly perfect gases, mutually interpenetrate. I now proceed to consider the mutual behaviour, when in contact with one another or with a permanent gas, of those gases which are comparatively near their point of condensation, and which at ordinary temperatures deviate sensibly from Mariotte's law. As vapours are not different in kind from perfect gases, we might

anticipate that the phenomena of their mutual interpenetration should follow a law not very different from that which applies to the latter. This we shall see to be the case. Dalton's law, however, requires certain modifications in the case of vapours, which are doubtless dependent on the imperfect gaseous character of these fluids.

Until 1836 it was supposed that vapours deposit themselves similarly in gases and in one another as in vacuo, with this difference only, that a sensible time elapses before equilibrium is established, while in the latter the process is almost instantaneous. In this year Magnus showed experimentally that when the vapours of two fluids which mutually dissolve one another are brought into contact, they do not follow Dalton's law.* A direct consequence of this law is, that the elastic force of a mixture of two vapours is equal to the sum of the elastic forces of these vapours, if existing separately in the given space. Magnus, on the contrary, found a mixture of the vapours of two fluids mutually soluble in one another, to have a tension less than the sum of the tensions of the component vapours at the same temperature, and that the tension of such a mixture is dependent upon the proportions in which the vapours are present. Ether was introduced into the vacuum of a barometer tube, and after the maximum tension of the vapour had been obtained, alcohol was added. The tension of both vapours together was seen to be less than that of the ether alone, and as the quantity of alcohol was increased, the tension became nearly equal to that of alcohol alone at the existing temperature. The same was observed when, instead of alcohol, oil of turpentine, or, instead of ether, bisulphide of carbon was used, or when alcohol was first introduced into the tube, and water afterwards added. Magnus did not detect any deviation from Dalton's law in mixtures of the vapours of two fluids which do not mutually dissolve each other.

We are indebted to the recent investigations of Regnault for the most valuable information we possess on this subject. The following are the results at which he arrived :—†

1. With reference to the mixture of vapours with permanent gases, he found that the elastic force of aqueous vapour in air and in nitrogen was invariably less than its tension in vacuo. The tension of aqueous vapour being exceedingly feeble at low temperatures, he extended his investigation to ether, bisulphide of carbon, and benzine. These experiments confirmed the previous results, as did also a number in which ether was evaporated into hydrogen and carbonic acid respectively. The explanation of these facts he considers to be as follows :—" When a volatile

* Pogg, *Annalen*, vol. xxxviii. p. 181.

† *Annales de Chimie et de Physique*, vol. xv.

liquid in a limited space is in contact at the same time with a gas which fills this space, and with the substance of which its walls are composed, it has a tendency to produce vapour until the tension of the latter shall be equal to that acquired by the vapours in vacuo at the same temperature. But this vapour comes in contact with the walls of the vessel, which by their hygroscopic affinity condense a portion of it on their surface. The space is consequently below saturation as long as its walls have not condensed the quantity of vapour necessary to satisfy their attractive action upon the vapour. It will be understood, therefore, that if this stratum which remains adherent to the wall (which cannot on the vertical portions increase indefinitely in thickness) be too thin to neutralize the attractive action of the wall upon the vapour, the space must lose vapour; and if the lost vapour be not supplied with the same rapidity by the excess of liquid, the elastic force must always appear less than observed in vacuo.* This explanation agrees with the observation, that after refrigeration or compression the condensed liquid becomes more abundant and the tension of the vapour increases, approaching more nearly to that in vacuo. With ether in hydrogen and in carbonic acid absolute equality has even been obtained, immediately after a reduction in the volume of the mixture. Taking these facts into consideration, we may altogether conclude that Dalton's law is true in the case of mixtures of gases and vapours, and would probably be strictly verified if we could allow the interpenetration to proceed without disturbance from bodies immersed in these fluids.

2. Mixtures of vapours emitted by volatile substances mutually dissolved or superimposed. In mixtures of vapours of volatile substances not mutually soluble, the elastic force of the mixtures is very nearly equal to the sum of the tensions given by the separate substances. This, however, is the only case in which Dalton's law is verified; for both in mixtures of substances which are partially soluble in one another, and of those which are mutually soluble in every proportion, the mixtures—far from exhibiting a tension equal to the sum of the tensions of the separate vapours—do not generally possess a tension equal to that which would be produced by the most volatile of the two fluids.

III.—*Mutual interpenetration of liquids.*—Unlike gases and vapours, liquids do not all mutually interpenetrate so as to produce a uniform mechanical mixture. While some are mutually soluble in every proportion, others mix sparingly with certain liquids, though in other liquids they may be indefinitely soluble. Thus water and alcohol mix in every proportion, while the

* Comptes Rendus, Aug., 1854.

fixed oils are quite insoluble in water, and sparingly soluble, with one exception, in alcohol, but freely though not indefinitely soluble in ether and the essential oils. From this and other obvious points of difference it might *a priori* be anticipated, that the laws of the interpenetration of liquids should be less simple or more difficult of discovery than those applicable to gases and vapours.

Several able and laborious experimental investigations of the subject have recently been made; but these have hitherto failed to place the inquiry in such a satisfactory state as that in which exists the corresponding one with regard to gases. Many valuable facts and principles have, however, been established, of which I shall proceed to give a brief account.

The first observations which appear to have been made were those of Parott* in 1811, but no progress was made towards constituting the subject a branch of physical science until 1849, when Graham published a paper on the "diffusion of liquids."

Graham's experiments were confined to aqueous solutions of various substances, and were conducted by the aid of the following apparatus:—A set of phials were cast in the same mould, and further adjusted to a uniform size of aperture by grinding. These phials were 3·8 in. high, with a neck 0·5 in. deep, and having an aperture of 1·25 inches in diameter; the capacity to the base of the neck was 2080 fluid grains. For each diffusion phial, as these were termed, a plain water jar was provided 4 inches in diameter and 7 inches deep.

In making an experiment the diffusion phial was filled with the liquid under investigation to the distance of 0·5 inch from the ground surface of the lip. The neck was then cautiously filled up with distilled water or an aqueous solution, as the case might be, the utmost care being taken to avoid agitation. After the phial had been placed in the water jar, the latter was filled with the same liquid as the neck of the phial, so as to cover the aperture of the phial to the depth of an inch, which required about 20 ounces of water. The liquid in the diffusion phial thus communicated freely with about five times its volume of a liquid differing from it—the atmosphere which invites diffusion. The process of diffusion is interrupted by closing the mouth of the phial by a plate of ground glass, and raising the former out of the jar. The amount of the substance diffused ("diffusion product," or "diffusate") is ascertained by evaporating the contents of the jar to dryness for the solids, and by neutralizing the liquid with a normal alkaline solution for the acids.

It will appear from all the experiments noticed under this head

* Treatise on Physics.

of our subject, that the mutual interpenetration of liquids is a very slow process compared with that of gases. The same remark applies indeed to the liquid diffusion of gases, as preliminary experiments by Graham show. Two 8-oz. bottles were selected, of which the mouths were 1·2 in. in diameter. These were filled, one with pure water, the other with carbonic acid water; the former was then carefully inverted over the latter, the two liquids being thus placed in free communication with one another. After five days the gas was found to be distributed between the two bottles in the following proportion—

In upper bottle,	1·63
In lower bottle,	8·44

Thus the time occupied by the liquid diffusion of carbonic acid, bears a ratio to that required for its gaseous diffusion quite as great as the ratio of days to minutes.

I now proceed to notice in order of succession the results obtained by Mr. Graham:—

1. The diffusion from solutions of the same density and temperature is not equal, but highly variable. This is shown by the following experiments, among others. The process of diffusion was continued for 27 days.

	Point. Deg. Fahr.	Diffusion product.	
		In grains.	Ratio.
Chloride of sodium,	225·5	269·80	100·
Nitric acid,	227·	581·20	215·42
Sulphuric acid,	223·	455·10	168·68
Chloride of potassium (density 1778),	221·	320·30	118·71
Bisulphate of potash,	216·	319·00	118·23
Nitrate of soda,	220·	260·20	96·44
Sulphate of magnesia,	214·	95·87	35·53
Sulphate of copper	213·5	77·47	28·71

Density 1·2. Temp. 66° Fahr.

The following experiment establishes the same remark, as applied to solutions containing equal quantities of anhydrous matter:—

DIFFUSION OF SOLUTIONS CONTAINING 20 PER CENT. OF ANHYDROUS MATTER,
AT 60°·5, FOR EIGHT DAYS.

Substance in Solution.	Diffusate.
Chloride of sodium,	58·68
Sulphate of magnesia,	27·42
Nitrate of soda,	51·56
Sulphate of water (HO, SO ₃),	69·32
Crystallized cane sugar,	26·74
Fused cane sugar,	26·21
Starch sugar,	26·94
Treacle of cane sugar,	32·55
Gum-arabic,	13·24
Albumen.	3·08

2. In experiments with 1, 2, 3, and 4 parts of chloride of sodium to 100 of water, it was found that the diffusion product was proportional with the limits of experimental error, to the quantity of salt in solution.

The numbers are as follows (time of diffusion being eight days):—

Proportion of Salt to 100 of Water.	Diffusion Product in Grains.		
	At 39°·6	At 52°·5	At 67°·
1,	2·63	2·78	3·50
2,	5·27	5·54	6·89
3,	7·69	8·37	9·90
4,	10·00	11·11	13·60

The same result was also obtained from observations with other salts, where the quantity of salt dissolved was not more than four or five per cent. Concentrated solutions will probably exhibit some deviation from the exact proportion, just as in the case of highly compressed gases, there is probably a departure from the normal law of diffusion.

It will be noticed, further, that in the above table the numbers expressing the diffusion products increase with the temperature, and, as far as can be determined by three observations, in direct proportion to the temperature.

In these and all subsequent experiments weak solutions were employed, and the process was continued for a comparatively short period of seven or eight days, in order to exhibit the initial diffusion. A more accurate comparison can thus be made between substances whose diffusibilities vary considerably; the influence of diminished rapidity of diffusion, consequent upon a decrease in the strength of the solution, being still small. The conditions also, in consequence of the large volume of water in the water jars, approach those of diffusion into an unlimited atmosphere.

3. There are classes of substances nearly if not quite equidiffusive, which coincide in many cases with the isomorphous groups, but are, generally speaking, more comprehensive than the latter. The following tables may serve as examples:—

TABLE I.

HYDROCHLORIC, HYDRIODIC, AND HYDROBROMIC ACIDS.

1.—Diffusion from 2 per cent. solutions at 51° Fahr.

	Diffusate.	Ratio.
Hydrochloric acid,	15·04	= 100·
Hydriodic acid,	15·11	= 100·46

2.—Diffusion from 2 per cent. solutions at 59°·7 Fahr.

Hydrochloric acid,	16·55	= 100·
Hydrobromic acid,	16·58	= 100·18

The similarity of diffusion in these is very marked, the ratios being nearly 100, 100·46, and 100·18. Nitric acid coincides very closely with this group when dilute solutions are employed.

TABLE II.

CHLORIDES, IODIDES, AND BROMIDES OF POTASSIUM AND SODIUM.

1.—2 per cent. solutions in 5·716 days, at 59°·8.

	Grains.	Ratio.
Chloride of potassium, . . .	12·24	100·
Bromide of potassium, . . .	12·46	101·80
Iodide of potassium, . . .	12·51	102·21

2.—2 per cent. solutions for 7 days.

Chloride of sodium at 60°, . .	12·15	100·
Bromide of sodium at 59°·8, . .	11·93	98·27
Iodide of sodium at 59°·8, . .	12·18	100·33

TABLE III.

1 per cent. solutions for 11·43 days, temp. 50°·8.

	Grains.	Ratio.
Chloride of calcium, . . .	6·51	100·
Chloride of manganese, . . .	6·63	101·85
Nitrate of magnesia, . . .	6·49	99·69
Nitrate of copper, . . .	6·44	98·92

TABLE IV.

1.—1 per cent. solution in 11·43 days, at 64°·1.

	Grains.	Ratio.
Nitrate of baryta, . . .	7·73	100·
Nitrate of lime, . . .	6·66	100·78

2.—1 per cent. solutions in 11·43 days, at 51°·5.

Nitrate of baryta, . . .	6·73	100·
Nitrate of strontia, . . .	6·79	100·78

TABLE V.

1 per cent solution in 7 days, temp. 64°·5.

	Grains.
Nitrate of potash,	3·72
Nitrate of ammonia,	3·75
Chloride of potassium,	3·88
Chloride of ammonium,	3·89
Chlorate of potash,	3·66

When we take into account the difficulties of experimenting, these coincidences must be considered very close. It must be remarked, however, that when solutions of greater density were employed, the divergences were much more considerable.

4. It now became an object of great importance to determine the relation between the diffusibility of the various equi-diffusive groups, so far as these had been ascertained. As might have been anticipated, it was found that the rate of diffusion of any

given liquid diminished with the time the process had continued. This was obviously due to the upper strata of solution in the diffusion jar becoming weaker, and the denser layers nearer the bottom presenting more resistance, as it were, to the diffusion of the dissolved substance into the water jar. Of this, however, we shall have more to say hereafter. This observation renders inadmissible a comparison between the diffusion products of two liquids having unequal rates of diffusion, and exposed during equal times. The liquid of smaller diffusibility will not, in the same time, advance so far as a more diffusible one, and hence the diffusion of its rate will not be so great as that of the latter. The result of this will be that in comparison with the latter, the diffusion of the former will be given in excess. The comparison will, however, be perfect if the same point in the progression has been obtained in both, or, which is the same in effect, if times of equal diffusion can be found.

Graham, recurring to the analogy of the mutual interpenetration of gases, expected that if a simple relation should be found, it would not be exhibited by the times of equal diffusion, but by the squares of those times. The results, so far as the investigation was prosecuted, are extremely interesting:—

Days.	Square of Times.	Temperature.	Solution.	Per cent. of Salt in Solution.	Diffusion Product.
{ 3.5 ... 1 ...	47.2 ...	Nitrate of potash,	...	3.5 ...	100.
{ 4.95 ... 2 ...	47.3 ...	Sulphate of potash,	...	3.5 ...	100.
{ 7. ... 1 ...	48.6 ...	Nitrate of potash,	...	6.04 ...	100.
{ 9.9 ... 2 ...	49.1 ...	Sulphate of potash,	...	6.20 ...	102.65
{ 9.9 ... 2 ...	49.1 ...	Chromate of potash,	...	6.29 ...	104.14
{ 10.5 ... 1 ...	48. ...	Nitrate of potash,	...	8.74 ...	100.
{ 14.85 ... 2 ...	48.6 ...	Sulphate of potash,	...	8.79 ...	100.57

A solution containing 1 per cent. of hydrate of potash was diffused for 4.95 days against a 1 per cent. solution of nitrate of potash for 7 days, at the temperatures of $53^{\circ}.7$ and $53^{\circ}.6$ Fahr., respectively. The diffusion products were:—

Hydrate of potash,	3.06 grains, = 100.
Nitrate of potash,	3.11 grains, = 98.2

But the squares of the times are in the ratio of 1:2. Wherefore for the salts of potash we appear to have a very close approximation to the following simple series of squares of equal diffusion-times:—

	Square of Times of equal Diffusion.
Hydrate of potash, 1
Nitrate of potash, 2
Sulphate of potash, 4

Experiments at $39^{\circ}7$ with nitrate and hydrate of potash for the selected times, gave mean diffusion products of 100 and 100.35, respectively, which concur with the observations at a higher temperature, proving that the times of equal diffusion have been properly chosen.

Similar experiments were made with salts of soda, which showed the times of equal diffusion of nitrate of soda and sulphate of soda to be as 1 to 1.4142, or as 1 to $\sqrt{2}$.

Further experiments were now undertaken with a view to determine the relation between salts of potash and salts of soda. It appeared probable, from previous results, that the times of equal diffusion of corresponding salts of potash and soda, were as $\sqrt{2}$ to $\sqrt{3}$. This was proved by the following experiment, in which 1 per cent. solutions were employed:—

	Temperature.	Time in Days.	Square of Times.	Diffusion Product in Grains.
Nitrate of potash, .	56.1	7	2	6.75
Nitrate of soda, .	55.7	8.57	3	6.78
Sulphate of potash, .	55.4	9.90	4	6.78
Sulphate of soda, .	55.4	12.125	6	6.72
Carbonate of potash, .	55.4	9.90	4	6.56
Carbonate of soda, .	55.4	12.125	6	6.54

Another numerous series of experiments was made at a much lower temperature, with the object of discovering more of the same relations. Solutions of 1 and 2 per cent. were chosen, and a mean temperature maintained of $39^{\circ}7$. The results are given in the following table:—

	Time in Days.	Square of Times.	Diff. Product of Cell in 2 per cent. Sols., and 2 Cells in 1 per cent. Sols.
Chloride of potassium, .	9	2	6.73
Nitrate of soda, .	11.022	3	6.81
Chloride of sodium, .	11.022	3	6.655
Sulphate of soda, .	15.589	6	6.485
Sulphate of magnesia, .	25.456	16	6.565

Mr. Graham proposes terming the numbers proportional to the squares of times of equal diffusion, *solution densities*. I have already shown, that the squares of the times of equal diffusion of gases are proportional to their densities. Hence we may ascribe an analogous density to liquids. Subjoined is a table containing the squares of "solution densities," which have hitherto been ascertained by direct experiment:—

	Solution Density.
Hydrate of potash,	1
Nitrate of potash,	
Chloride of potassium,	2
Nitrate of soda,	
Chloride of sodium,	3
Sulphate of potash,	
Carbonate of potash,	4
Sulphate of soda,	
Carbonate of soda,	6
Sulphate of magnesia,	16

From what has already been stated with reference to isomorphous salts, it will be obvious that all these "solution densities" are proper to many substances: in fact, each is applicable generally to all isomorphous substances belonging to the same group as the substance in the table, opposite which the number expressing that solution density stands.

5. With reference to the analogous question in the mutual diffusion of gases, as well as for other reasons, it became peculiarly important to find whether the diffusion of one liquid into water already charged with a different substance, was the same as the diffusion of the former into pure water.

An experiment similar to that already mentioned on the liquid diffusion of carbonic acid was made, with this difference, that instead of pure water in the upper bottle, water holding in solution three-fourths of its volume of nitrous oxide was employed. The carbonic acid which had diffused into the upper bottle from the lower was .231 when the upper contained pure water, and .229 when the water contained three-fourths of its volume of nitrous oxide to one remaining undiffused in the lower bottle, in both cases. It appears then that the liquid diffusion of carbonic acid is not retarded by nitrous oxide, any more than its gaseous diffusion by gaseous nitrous oxide. The same was established for solids dissolved in water.

Three similar and equal solution phials were taken, each filled in the ordinary way with a 4 per cent. solution of carbonate of soda. One was placed in a water jar containing pure water; the other two in similar jars, each containing a quantity equal to that in the former jar, of a 4 per cent. solution of chloride of sodium. The diffusion proceeded for eight days at a temperature of 64° Fahr., with the following result:—

Experiment I.—Diffusion product into water,	9.06 grains.
" II.—Diffusion product into chloride of sodium,	8.82 "
" III.—Diffusion product into chloride of sodium,	9.10 "

Thus the chloride of sodium appears to have no sensible effect in retarding the diffusion of carbonate of soda. In a similar

experiment an equal quantity of sulphate of soda was substituted for the chloride, when the diffusion products were respectively 7.84 and 7.82 grains, into water and solution of sulphate of soda. This shows a small reduction in the latter case, but not more than might be due to experimental error. Nitrate of potash also, is not sensibly resisted by nitrate of ammonia, even though these are strictly isomorphous salts.

These experiments were made upon dilute solutions, and it is not improbable that the results may be considerably modified when the solutions approach saturation; just as we have seen to be the case in gases near their points of condensation. Although different substances in solution possess normal relations in density, these relations may be concealed by concentration, so as to become not directly observable.

6. Inequality of diffusion supplies a method for the separation to a certain extent of mixed salts into their components.

Equal weights of the anhydrous carbonates of soda and potash were dissolved in a weight of water equal to five times that of the mixture. This was diffused from a 1000 grain bottle of 1.1 inch aperture into 6 ounces water, the process continuing for nineteen days. The salts diffused out were in the proportion of:—

Carbonate of potash,	36.37
Carbonate of soda,	63.63

In twenty-five days the diffusate contained the salts in the proportion of 35.2 to 64.8. The salts in sea-water were partially separated in a similar manner, the quantity of magnesia originally in the water being 6.01 per cent. of the whole salts held in solution; while after diffusion for eight days the magnesia was only 4.09 per cent. of the whole salts diffused into the water jar. The magnesia also in the liquid remaining in the diffusive cell must be relatively large in amount.

Mr. Graham proposes to explain the varying composition of the Dead Sea at different times of the year, by the unequal rate of diffusion of different salts from the dense liquid nearer the bottom of the lake into the superior stratum of fresh water brought down during the rainy season. The composition of the water at the surface must vary greatly, as this process of diffusion is more or less advanced.

7. In certain cases diffusion is capable of effecting the decomposition of chemical compounds, as is shown by the following experiments:—

A solution of bisulphate of potash saturated at 68° Fahr., was submitted to the diffusion process for fifty days. At the end of that period crystals of neutral sulphate were found in the solution cell, and the contents of the water jar were:—

	Grains.	
Sulphate of potash,	20.37	} Bisulphate of potash.
Sulphate of water,	11.47	
Sulphate of water,	12.77	

It thus appears that bisulphate of potash undergoes decomposition to such an extent, that the acid and sulphate of potash diffused are to one another in the ratio of about two equivalents of the former to one of the latter.

A similar experiment was made with a 4 per cent. solution of common alum (double sulphate of alumina and potash). The diffusion products consisted of:—

Alum,	5.33
Sulphate of potash,	2.15

After the diffusion of a 4 per cent. solution of ammonio-sulphate of copper, there were found in the water jar:—

Sulphate of copper,	.81
Sulphate of ammonia,	5.46

The abundant formation of sulphate of ammonia shows ammonio-sulphate of copper to be largely decomposed by diffusion.

We have now completed this brief abstract of Graham's principal results, which constitute the most valuable part of experimental information on this subject. His method of comparing the relative diffusibility of different salts, however, presents very great practical difficulties to an extension of that most important part of the inquiry.

A principle introduced by Dr. Fick of Zürich, furnishes a direct method of determining diffusion coefficients, which obviates the difficulties attending that of Graham. According to Fick's law, the transfer of the dissolved matter and water occurring in a unit of time between two elements of space filled with differently concentrated solutions of the same substance is, *ceteris paribus*, directly proportional to the difference of concentration, and inversely proportional to the distance of the elements from one another.* This law is identical with that upon which Fourier founded his theory of the conduction of heat, and which has been extended to the conduction of electricity—the only change being the substitution of “difference of concentration” for “difference of temperature” in the first, and “difference of potential” in the second.

Assuming then Fourier's differential equation expressing the strength of a current of heat, we have, *mutatis mutandis*, from the foregoing law for the diffusion current, the formula—

$$\frac{\delta y}{\delta t} = -k \left(\frac{\delta^2 y}{\delta x^2} + \frac{1}{Q} \cdot \frac{\delta y}{\delta x} \right)$$

* Poggendorff's *Annalen*, vol. xciv. p. 59.

which he interpreted thus:—In a column of solution, y represents the concentration of an element, and is also a function of the height, x , of this element above a horizontal plane of reference. The condition must, however, be introduced, that y diminish as x increases, otherwise the diffusion would be interfered with by gravity; k is a constant depending upon the liquid diffused; and Q the horizontal section of the column at a height, x , from the plane of reference.

If the solution at the base of the column be kept concentrated, and diffusion allowed to take place from the upper end into a relatively infinite reservoir of water, it is obvious that after a sufficient time has elapsed, the diffusive current will become constant; that is, it will no longer alter the density of the strata through which it passes, but will in a given time expel from each stratum as much of the substance held in solution as enters that stratum in the same time. Hence in this case $\frac{d y}{d t} = 0$, and if the column be cylindrical, and therefore of constant section, $d Q = 0$. Wherefore equation (1) becomes—

$$Q = -k \frac{d^2 y}{d x^2} \text{ or } \frac{d^2 y}{d x^2} = 0,$$

which by integration gives the expression $y = ax + k$. This equation contains the following proposition:—"If in a cylindrical vessel dynamic equilibrium shall be produced, the differences of concentration of any two pairs of strata must be proportional to the distances of the strata in the two pairs," or, in other words, the density is graphically represented by a straight line.

This law was very completely verified by Fick for a solution of chloride of sodium, and the *prima facie* probability of its truth is so great, that we may, I think, with safety extend it to all others. Nothing more then remains, in order to determine the diffusion of a solution into water, than to ascertain the value of k for that solution; k may be called the diffusion coefficient, and is that quantity of dissolved substance which in a unit of time passes per unit section from one stratum into the immediately adjacent one, when the rate of diminution of concentration, or $\frac{d y}{d x}$ = a constant.

Simmler and Wilde have proposed other methods of determining diffusion coefficients; but as no results obtained by these methods have yet been published, I shall content myself with a simple reference to them.*

The phenomena of osmose, or the passage of liquids through porous septa, have by Fick and others been ascribed to the diffusive force of those liquids. The recent investigations of Graham lead, however, to the conclusion that osmose depends chiefly on the chemical action of the liquid on the septum.* Indeed it is probable that the osmose between water and saline solutions consists, not in the mutual interpenetration of the two liquids, but in the passage of minute particles of the salt in one direction, and of pure water in the other. However this may be, the laws of the mutual interpenetration of liquids are so much interfered with, as to remove the question beyond the strict limits of the subject of this paper.

The property of diffusion in fluids, like the corresponding property in gases, is of the greatest importance in the economy of nature. It operates, though complicated by osmose, in the motions of fluids in animal and vegetable tissues. The nutrition of plants must be aided greatly by the diffusion of the soluble matter of the soil upwards into the rain water. The salts of potash and ammonia which are most required for vegetation, possessing the greatest diffusibility, will rise first. The special applications of this force, and the functions which the variety of diffusive powers are intended to subserve, are, however, still imperfectly understood.

Much, indeed, remains to be done in this inquiry. Besides the determination of diffusion coefficients, and an investigation of the relations, if any, which subsist between these and the atomic equivalents or other physical properties of the substances; and the exact effect of external circumstances, such as temperature on the process—there is a total absence of information on the mutual interpenetration of all fluids not capable of solution in water.

IV. *Absorption*.—All gases are absorbed by liquids on which they exert no chemical action, in a greater or less degree, the quantities depending upon:—

1. The nature of the gas and absorbing liquid.
2. The temperature of the absorbing liquid.
3. The pressure to which the gas is subjected during absorption.

The volumes of a particular gas absorbed at different temperatures, can only be determined empirically. Their variation is generally a diminishing function of the temperature, but the ratio depends upon the special nature of the absorbed gas.

Between the tension of a gas during absorption, and the volume of gas absorbed by a given quantity of a specific liquid, there exists, however, a very simple relation. This was first

* Phil. Trans., 1855, p. 177; Chem. Soc. Qr. Journal, viii. 43.

announced by Dr. Henry in 1802, and is known as the hypothesis of Dalton and Henry.* This law is that "The quantity of any gas absorbed, increases in direct proportion to the tension of the gas during absorption." The experiments which were made with a view to the verification of the law, were, however, of an extremely inaccurate character, until Bunsen's recent and very careful examination of the subject.†

In order to compare Bunsen's results with the law of Dalton and Henry, it will be necessary, on account of the method adopted by the former, to consider the absorption of different gases at the same temperature and under the same pressure.

The volume of a gas, at a temperature of 0° Cent., under a pressure equal to $0^{\text{m}}\cdot76$ ($29\cdot9218$ inches) of mercury, absorbed by the volume of a liquid, is termed the absorption coefficient of that gas, with reference to the given liquid.

Let, then, V represent the volume of a gas before absorption, at 0° Cent., and under a pressure $= P$ expressed in metres, and let V' be the volume after absorption of the same gas, at 0° Cent., and under a pressure $= P'$, then the amount of gas absorbed by a limited volume of liquid will be

$$= \frac{VP - V'P'}{0\cdot76}$$

According to the law of Dalton, the quantity absorbed, when the pressure during absorption is $0^{\text{m}}\cdot76$, will be

$$V \frac{P}{P'} - V'$$

Hence, if h be the volume of absorbing liquid, and a the coefficient of absorption—

$$a = \frac{1}{h} \left(\frac{VP}{P'} - V' \right) (1).$$

This is the formula employed by Bunsen in the determination of absorption coefficients. The amount of absorption was determined in the case of the more soluble gases, such as hydro-sulphuric acid, ammonia, &c., by saturating the liquid with the gas, and afterwards determining by volumetric or by weighed analyses the quantity of gas in the liquid.

For the less soluble gases, a more complex method was found necessary. The apparatus employed was termed an absorptiometer, of which the following is a general description:—

A carefully graduated tube, closed at the top, is inclosed within a wider tube, whose axis corresponds with that of the first.

* Phil. Trans., 1802.

† Phil. Mag. [4], vol. ix., 116, 181.

The outer cylinder is closed at its upper end by a lid, in the middle of which is an india-rubber cushion pressing firmly on the top of the inner tube, and keeping it in a fixed position during the violent agitation necessary to promote the process of absorption. The inner tube, by a peculiar arrangement, can either be firmly closed at its lower end, or opened so as to communicate with a volume of mercury at the bottom of the outer cylinder in which it stands.

In making an experiment, the space between the two cylinders is filled up, over the mercury, with water, by means of which any required temperature may be maintained in the inner tube. A volume of the gas to be examined is then collected over the mercury with the same precautions in reading off, &c., as in the ordinary measurement of gases. A measured volume of liquid employed for absorption, having been freed from air, is next admitted into this tube, and the tube fixed in its place in the larger cylinder. The pressure within and without having been equalized by opening the tube, it is again closed, and the whole apparatus rapidly agitated for a short time. The agitation is repeated many times, the tube being opened between each, until no further change of volume is perceptible. Finally the observations necessary for reducing and measuring the remaining gas are made.

The following table exhibits the coefficients obtained by these means, together with the quantities absorbed for every 5° cent., the millimetre being adopted as unit. The values are partially calculated by means of an interpolation formula, which is of the form—

$$Q = a - ct + c_2 t^2$$

and is obtained in each case by the combination of a number of experiments:—

	Oxygen.			Hydrogen.		
	In Water.	In Alcohol.		In Water.	In Alcohol.	
0°C. ...	0.04114	0.06925
5 ...	0.03628	0.06853
10 ...	0.03250	...	0.28397	0.0193	...	0.06786
15 ...	0.02989	0.06725
20 ...	0.02838	0.06668
25 ...	—	0.06616
	Nitrogen.			Nitrous Oxide.		
	In Water.	In Alcohol.		In Water.	In Alcohol.	
0°C. ...	0.02035	...	0.12634	1.3052	...	4.1780
5 ...	0.01794	...	0.12440	1.0954	...	3.8442
10 ...	0.01607	...	0.12276	0.9196	...	3.5408
15 ...	0.01478	...	0.12142	0.7778	...	3.2678
20 ...	0.01403	...	0.12038	0.6700	...	3.0253
25 ...	—	...	0.11964	0.5962	...	2.8133

Carbonic Oxide.					Carbonic Acid.				
		In Water.	In Alcohol.				In Water.	In Alcohol.	
0°C.	...	0.03287	0.20443	...			1.7967	...	4.3295
5	...	0.02920					1.4497	...	3.8908
10	...	0.02635					1.1847	...	3.5140
15	...	0.02432					1.0020	...	3.1993
20	...	0.02312					0.9014	...	2.9465
25	...	—					—	...	2.7558

Marsh Gas.					Olefiant Gas.				
		In Water.	In Alcohol.				In Water.	In Alcohol.	
0°C.	...	0.05449	...	0.52259	...	0.2563	...	3.5950	...
5	...	0.04885	...	0.50861	...	0.2153	...	3.3234	...
10	...	0.04372	...	0.49535	...	0.1837	...	3.0859	...
15	...	0.03909	...	0.48280	...	0.1615	...	2.8825	...
20	...	0.03499	...	0.47096	...	0.1488	...	2.7131	...
25	...	—	...	0.45982	...	—	...	2.5778	...

Sulphurous Acid.					Hydrosulphuric Acid.				
		In Water.	In Alcohol.				In Water.	In Alcohol.	
0°C.	...	79.789	...	327.80	...	4.3706	...	17.891	...
5	...	67.485	...	251.24	...	3.9652	...	14.776	...
10	...	56.647	...	190.02	...	3.5858	...	11.992	...
15	...	47.276	...	144.13	...	3.2326	...	9.539	...
20	...	39.374	...	113.56	...	2.9053	...	7.415	...
25	...	32.786	...	98.33	...	2.6051	...	5.623	...
30	...	—	...	—	...	—	...	—	...
35	...	22.489	...	—	...	2.0799	...	—	...
40	...	18.766	...	—	...	1.8569	...	—	...

		Chlorine, In Water.	Nitric Oxide, In Water.	Ammonia, In Water.	Methyle Gas, In Alcohol.
0°C.	...	—	0.31606	1049.60	0.0871
5	...	—	0.29985	917.90	0.0720
10	...	2.5852	0.28609	812.76	0.0599
15	...	2.3681	0.27478	727.22	0.0508
20	...	2.1565	0.26592	653.99	0.0447
25	...	1.9504	0.25951	585.94	—
30	...	1.7499	—	—	—
35	...	1.5550	—	—	—
40	...	1.3655	—	—	—

It appears from this table that the quantity of gas absorbed generally increases with a diminution of temperature.

When a liquid is in contact with a mixture of gases, without acting chemically upon any of them, it absorbs of each gas a quantity directly proportional to the tension of that gas in the mixture, and to its coefficient of absorption. Now, we have shown in the former paper that the tension of one gas in another is the same as its tension in vacuo. Hence the tension of a mixture of gases is equal to the sum of the tensions of the constituent gases. If then a gaseous mixture of volume = V consist of

different gases, whose volumes are $v_1, v_2, \dots v_n$ respectively; and if the tension of the mixture be P , it follows that the pressure of the first gas will be $\frac{v_1}{V} P$; of the second, $\frac{v_2}{V} P$; and of the n th, $\frac{v_n}{V} P$.

These may be termed "partial pressures," as distinguished from the "total pressure" of the mixture, which is equal to the sum of the partial pressures.

The law of absorption assumed, enables us to determine, from the coefficient a and pressure P , the quantity of gas g absorbed by the volumes of liquid. This is obviously expressed by the equation—

$$g = a h \frac{P}{0.76} \quad (2)$$

Applying this to the case of the above mixture; if $a_1, a_2, \dots a_n$ be the coefficients of constituent gases, and $g_1, g_2, \dots g_n$, the quantities absorbed of each respectively, we obtain—

$$g_1 = a_1 h \frac{P v_1}{0.76 V}$$

$$g_2 = a_2 h \frac{P v_2}{0.76 V}$$

.

$$g_n = a_n h \frac{P v_n}{0.76 V}$$

These formulæ are strictly true only when the proportions of the different gases composing the mixture are not altered by unequal absorption; that is, when the volume of gas is infinitely great compared with that of the liquid in contact with it.

Let us now consider the case in which the relations of two gases are to be determined by an absorptiometric experiment.

Let V represent the volume of the mixture before absorption under pressure P and V_1 the volume unabsorbed under pressure P_1 . Let x and x_1 , respectively, represent the original and unabsorbed volumes of the first gas reduced to pressure unity. Also, let the absorption coefficients of the two constituent gases be respectively α, β .

The partial pressure of the first gas in the unabsorbed mixture is then $\frac{x_1}{V_1}$. The volume of the gas unabsorbed under this pressure would be αh , and under pressure unity it is therefore

$$\alpha h \frac{x_1}{V}$$

hence,

$$x = x_1 \left(1 + \frac{a}{V^1} h \right)$$

and

$$x_1 = \frac{x}{1 + \frac{a}{V^1}}$$

wherefore,

$$\frac{x_1}{V^1} = \frac{x}{V^1 + a h}$$

If for the second gas, y, y^1 , represent the same values as x, x^1 for the first gas, then the tension (or partial pressure) of the second gas in the unabsorbed mixture is—

$$\frac{y_1}{V^1} = \frac{y}{V^1 + \beta h}$$

Therefore, the total pressure of the unabsorbed mixture is expressed by the equation—

$$P^1 = \frac{x}{V^1 + a h} + \frac{y}{V^1 + \beta h}$$

If, in this expression we take $h = 0$, that is, when no absorption has taken place, then $P^1 = P$ and $V^1 = V$, whence we obtain—

$$P = \frac{x}{V} + \frac{y}{V}$$

Dividing the last two equations by P^1 and P respectively we have—

$$1 = \frac{x}{(V^1 + a h) P^1} + \frac{y}{(V^1 + \beta h) P^1}$$

and

$$1 = \frac{x}{VP} + \frac{y}{VP}$$

From these formulæ we readily find the proportions of the first and second gases in the mixture to be respectively—

$$\left. \begin{aligned} \text{and } \frac{x}{x+y} &= \frac{VP - (V^1 + \beta h) P^1}{(V^1 + a h) - (V^1 + \beta h)} \cdot \frac{(V^1 + a h)}{VP} \\ \frac{y}{x+y} &= \frac{(V^1 + \beta h) P^1 - VP}{(V^1 + a h) - (V^1 + \beta h)} \cdot \frac{(V^1 + \beta h)}{VP} \end{aligned} \right\} (3)$$

We have now made sufficient progress in this enquiry to apply Bunsen's experiments in their relation to Dalton and Henry's hypothesis. If a given gas be absorbed at the same temperature, and by the same volume of a liquid, but under two different pressures, P_0, P_1 , the amount of gas g_0, g_1 , absorbed in the two cases, respectively, is by equation (2)—

$$g_0 = \frac{a P_0 h}{0.76}$$

and

$$g_1 = \frac{a P_1 h}{0.76}$$

whence,

$$\frac{g_0}{g_1} = \frac{P_0}{P_1}$$

The following are the results of absorptiometric experiments upon carbonic acid—

I.

Temperature = 19°·9 C.

		P_0		g_0		$\frac{P_0}{P_1}$		$\frac{g_0}{g_1}$
1	...	0.7255	...	38.61	...	—	...	—
2	...	0.5245	...	27.24	...	1.38	...	1.42
3	...	0.5237	...	27.08	...	1.39	...	1.43
4	...	0.5231	...	27.28	...	1.39	...	1.42

II.

Temperature = 3°·2 C.

		P_0		g_0		$\frac{P_0}{P_1}$		$\frac{g_0}{g_1}$
1	...	0.5244	...	31.41	...	—	...	—
2	...	0.6467	...	38.66	...	0.8109	...	0.8125
3	...	0.6470	...	38.49	...	0.8105	...	0.8161

These results show, within the limit of experimental error, the truth of the law for carbonic acid.

The ratio of absorbed gas to pressure is also shown very clearly in cases where partial pressures occur; that is, when the variation of pressure is effected by dilution with another gas. By means of formula (3), the composition of mixtures of two gases can be calculated when the constants of each gas are determined. If this calculated composition agree with that found directly by eudiometric analysis, it may be concluded that the law upon which the formula is based is true, and also that it is applicable for partial as well as for total pressures. The

following exhibits a mean of three experiments, taken at random from the numerous results obtained by Bunsen :—

	Absorptiometric Analysis.	Eudiometric Analysis.
Hydrogen,	26·67	26·81
Carbonic acid,	73·33	73·19
	<hr/> 100·00	<hr/> 100·00

An equally close agreement was found between absorptiometric and direct analysis in mixtures of carbonic acid and carbonic oxide. Mr. Watt says that the law also holds in mixtures of sulphurous acid with hydrogen and carbonic acid, and of carbonic oxide, marsh gas, and hydrogen; but not with a mixture of equal volumes of chlorine and hydrogen, or of chlorine with twice or four times its volume of carbonic acid (*"Graham's Chemistry, Supplement,"* p. 651). Mr. Watt does not, however, cite his authority, and I have not met anywhere with an account of the experiments to which he refers. It may probably be assumed then, that the hypothesis of Dalton and Henry is true within ordinary limits, though it is also highly probable that in this law as in that of Mariotte, there exists a limit beyond which the regularity of the action is disturbed by the operation of varying molecular forces.

M. Bunsen gives an elaborate development of this law in its application to several problems of great interest and importance. A complete discussion of these would, however, exceed my limits, and I shall therefore merely notice those of more importance.

The quantitative composition of a gas, obtained by ordinary eudiometric analysis depends, almost entirely upon its supposed qualitative constitution. If, for example, a eudiometric experiment indicates the presence of marsh gas, it remains uncertain whether this is a mixture of equal volumes of hydrogen and methyle; or, if the presence of a mixture of marsh gas and hydrogen is indicated, it is quite uncertain whether that mixture consists of methyle and hydrogen, or of methyle, marsh gas, and hydrogen. Bunsen shows that in this and other similar cases all doubts are removed by absorptiometric experiments. When the absorption coefficients of gases are determined, three experiments are sufficient to determine the qualitative and quantitative composition of gaseous mixtures. There may be some cases, indeed, where the absorption curves of different gases approach, cut, or touch each other; and here a second experiment at a different temperature, or with another liquid, would be necessary. Hence, a determination of the amount of gases absorbed at various temperatures and by different liquids, becomes an important element in this species of analysis; for any number

of equations may thus be obtained, each of which possesses the value of an additional reagent.

In a similar manner, the alteration of constitution which a mixture of gases undergoes by contact with water may easily be determined by means of the absorptiometer. From the table of absorption coefficients given above, it will appear that inequalities of absorption of the component gases, may, under certain circumstances, induce very considerable error in eudiometric experiments, where gaseous mixtures are confined over water.

It is quite unnecessary that I should here enumerate the various ends which are subserved in nature by this property of absorption of gases. The absorption of air by the blood, of various gases by the juices of plants, and other applications, are sufficiently obvious. The purposes subserved by the inequalities of absorption for various gases and by different liquids, together with the effect of differences of temperature and pressure, are, however, still very imperfectly understood.

A remarkable illustration of the effect of inequality of absorption may be noticed in the phenomena of the oceanic atmosphere, which are, doubtless, to a considerable extent, dependent upon this cause. In the diminution of absorption, with increase of temperature also, we cannot fail to remark an endeavour of nature to increase the supply of nourishment to plants in cold climates, and to keep within limits, by a more sparing supply, the vegetation of the tropics.

The outline which I have now given of the present state of the enquiry into the "laws of the mutual interpenetration of fluids," shows that much remains for future research. The subject has, indeed, scarcely emerged from the field of natural history into that of physical science. Though it may not in itself appear so attractive as many other physical subjects, it is of especial interest and importance in its bearing upon the phenomena of osmosis, a subject on which I hope to make some remarks on a future occasion.

IV.—*Cases of Induration of the Brain.* By ALEXANDER ROBERTSON, M.D., Medical Officer, Town's Hospital, Glasgow.

ASSOCIATED with and evidently in a great degree resulting from that peculiar condition of the blood—that dyscrasia—which is induced by the abuse of alcoholic liquors, pathological anatomy reveals morbid conditions of most of the organs and tissues of the body. The liver may be found small, granular, and con-

tracted, in a state of cirrhosis; or, it may be, a transformation of its tissue has taken place, fat globules have in a greater or less degree replaced the proper secreting structure and general parenchyma, giving rise to a form of the "fatty" liver; or, perhaps, the gland is simply in a state of hyperæmia, with the hepatic tubes distended with epithelial cells, secreted in excess from the abnormal stimulus. The kidney may be found in similar conditions—congested and enlarged, with the tubuli uriniferi packed with exfoliated cells and exuded fibrin; increased in bulk, yellow or mottled in colour, owing to the fatty degeneration of the proper gland substance of the organ, especially its epithelial cells; small, hard, and atrophied, the proper secreting tissue having broken down and been discharged, while the other tissues, subservient to it, are shrunk and contracted. The heart and great vessels may have become the seat of steatosis with resulting cardiac hypertrophy and dilatation. The muscular system, voluntary or involuntary, may also have suffered from fatty degeneration; and even the osseous tissue may be similarly affected. The nervous system is involved in the general morbid change. Its tissue is hardened and atrophied, and sometimes the seat of an albuminoid development. The blood itself is deficient in fibrin, and often abounds in fat globules. But we seldom, even in confirmed drunkards, find all the organs in one or other of the conditions described; and, if so, the majority are only implicated in a minor degree. More frequently one, or at most two of the principal organs, exhibit marked evidence of diseased action. The liver probably suffers most frequently. The kidneys are also very often involved. The brain is seldom affected to so serious a degree as to induce a fatal result from a direct interference with its functions. These are more generally impaired or destroyed from the impure blood which circulates in the cerebral vessels, owing to the non-elimination of those effete products of the tissues which form the special secretions of the kidney and liver, according as either or both of these organs are implicated. In the cases which I am now about to record, the brain was the organ most seriously suffering, and the unfavourable termination was especially due to the cessation of its functions:—

Case I.—M. C., aged 19, millworker, admitted September 14th, 1860.

History and State on Admission.—For three years has led a very irregular life, and has been in the habit of drinking, especially whisky, to great excess. Has not menstruated for two and a half years. Two months ago began to suffer from marked debility, gradually increasing, and accompanied by cough and slight irregular pain in chest. About a month since eyesight

began to fail, and is steadily getting worse. Now, can with difficulty distinguish objects, unless they are prominent and close to her. Is stupid and confused, but answers relevantly simple questions, although usually only after they are repeated. Staggeres when walking, evidently from a loss of muscular power. Arms are also slightly paralyzed. In speaking, pronounces the words slowly, but more from stupidity than inability to articulate. Sensation appears dulled, but to what degree cannot be ascertained from the impairment of her mental powers. Making allowance for the last condition, hearing seems tolerably acute. Pupils are widely dilated, and respond slowly and imperfectly to the stimulus of light. Conjunctivæ are clear and not at all injected. Urine and stools are frequently passed in bed, but the sphincters are evidently not involved to a great extent, as she is occasionally cleanly in her habits. The urine is quite normal in character. Percussion and auscultation reveal nothing abnormal in chest, except a few mucous râles. Hepatic and splenic dulness are also natural. Pulse 84, full. General condition spare and anæmic. No evidence of syphilis. Her symptoms gradually grew more unfavourable. Amaurosis became complete. Paralysis was more developed. Stupor increased, and eventually subsided in coma, which speedily terminated fatally—death occurring a month after admission.

The treatment consisted chiefly in counter-irritation by blisters to the entire head, and, at first, purgation.

Post-mortem.—Calvarium and dura mater normal. Arachnoid slightly opaque, particularly in the course of the superior longitudinal sinus. Glandulæ Pacchioni unusually large. Pia mater injected, but not intensely so. A slight increase of fluid in cavity of arachnoid. A hard body, about the size of a small bean, gritty to the touch, and apparently of a calcareous nature, was found in left choroid plexus. The white substance was pale, and the brain generally less vascular than usual. The most marked morbid change was the uniform increase in density and toughness, so much so, that it required considerable force to pass the finger into either the grey or white substance. In fact, it resembled very much a brain that had been steeped for a few days in alcohol, only that there was little, if any, appreciable contraction.

Remarks.—In this girl of 19 there existed an induration of the brain, which is rarely found except in aged drunkards; but it differed in this respect, that it was unaccompanied by any decided atrophy. Possibly this apparent absence of atrophy may have resulted from an albuminoid development, compensating for the contraction of the normal tissue, and resulting from a degree of vascular overaction in the course of her disease. Such a development is believed to take place in hypertrophy of

the brain. But if such overaction existed it did not seem to have reached the inflammatory acme, as, with the exception of the slight opacity of the arachnoid, which appears to be almost normal in some instances,* there was no evidence of inflammation. How then are we to account for the induration? Probably partly to some increase of vascular action, but also very much to the direct action of alcohol in hardening the brain tissue, just as it does dead organic matter. The researches of Lallemand and his associates show, that at least a portion of the alcohol which may be absorbed from the stomach, is thrown off by the lungs unchanged, undergoing no decomposition in passing through the system. In cases of death from drunkenness it has also been found in the brain. It is to be expected then that, when thus unaltered, it should exercise to some extent its special properties on the organs with which it comes in contact, and especially on nerve tissue, so delicate in its organization.

Case II.—H. M., aged 50, labourer, admitted 19th December, 1860. Friends state that for many years he has been a confirmed drunkard. About six months since they observed that he had become very peculiar in his manner and irritable in his temper, even when he was not under the influence of alcohol. This change of disposition grew more marked, so that it was evident to all who knew him, that his mental powers were considerably impaired. His legs were noticed to be getting feeble, and he could walk only with much difficulty. About four or five days before admission he became very stupid and confused, dirty in his habits, inattentive to his wants, and also very hot and feverish.

State on Admission.—Talks incoherently, and cannot understand the simplest questions. Is unable to walk without assistance; rather shuffles than walks; cannot sit upright in bed without support. Motion of hands is weak and uncertain. Both sides of body are equally affected, and legs more than arms. Is slow in utterance, although there is no paralysis of speech. Right eye was lost by an injury many years since. Pupil of left is natural in size, but does not contract much on exposure to strong light. There is no increased heat of head. Urine abounds in phosphates, but contains no albumen. Habits are dirty, but evidently from unconsciousness, rather than from loss of power. Pulse 76, full; bowels rather constipated. No symptom of disease in other organs. Bowels were freely acted on by croton oil, and repeated blisters were applied to the head. About

* I have observed opacity of the arachnoid, along the course of the superior longitudinal sinus, in several instances of parties dying from chest or abdominal diseases, in whom there was no history of brain disorders; but they were almost all past middle age.

a fortnight after admission the stupor gradually grew less, and his mind became much clearer, although still weak and wandering. The sight of left eye and hearing were then observed to be considerably impaired. He grew more cleanly, but the general paralytic symptoms were not affected. His improvement was but short-lived, as about ten days afterwards a state of stupor and delirium supervened, accompanied by general pyrexia. Depletory measures were employed, followed by counter-irritation, by blistering to the head, and after some days he again partially emerged from this semi-comatose state. However, in about a fortnight these formidable symptoms recurred, and this time in a still graver form. They gradually increased in severity, and eventually terminated fatally. But it is to be observed, that a completely comatose condition did not precede death, although there was much of stupor and delirium. There was wanting the characteristic respiration, and he could be roused to slight consciousness till within a short time of death.

Autopsy.—Scalp, calvarium, and dura mater, were normal in character, except that the skull was rather deficient in diploë. The arachnoid was opaque and thickened over the upper part of the right cerebral hemisphere, and also over the left side of the cerebellum. Pia mater was generally injected. The lateral ventricles contained a small quantity of serous effusion. There was considerable softening of the left side of the cerebellum, and of the hippocampus minor of the left lateral ventricle. This condition was all the more marked from the general induration of the brain substance, which, though not so great as in the previous case, was still quite evident. Along with it there was an absence of the puncta vasculosa, and an increased paleness of the white substance.

Remarks.—In the progress of this case there were distinct remissions and exacerbations—conditions which did not exist in the preceding instance. The evidences of acute action in the limited brain softening, and the injection of the pia mater, account for these differences. But, excluding these, there was a close correspondence in the two cases between the leading symptoms and the pathological conditions. In the latter the thickening and opacity of the arachnoid, apparently of considerable standing, was probably associated with the general induration of the brain substance, and due to the action of the same cause.

Case III.—J. M., aged 60, baker, admitted 19th February, 1861. Has been a very dissipated man, having been in the habit, whenever he could get it, of drinking whisky in large quantities. Says that for many years, especially in winter, has been subject to cough and frothy expectoration, and that during the last three months he has occasionally been swelled a little

about the face, and sometimes over the greater part of the body. Has a dull, stupid, sleepy appearance, but replies to questions quite readily. General sensation and muscular power are weakened, but not greatly impaired. Sonorous and mucous râles are heard generally over chest. Heart's action is regular, but feeble. Percussion elicits normal sounds, except a slight increase of cardiac dullness. Abdomen is tumid, owing to flatulent distension of the bowels, and also to oedema of the integument. Hernial protrusions exist on a level with and above the umbilicus. They have manifestly taken place through the substance of the recti muscles. Skin of the back and also of thighs presents patches of psoriasis, with here and there portions which have been the seat of irregular deposits of dark pigment matter. Pupils equal and natural; appetite bad; bowels regular; urine albuminous, and presents small tube casts under the microscope; pulse 110, small and weak. Smart purgation was produced by means of the compound jalap powder, and he was ordered tinct. ferri muriatis, in doses of twenty drops thrice daily. He had also an ordinary expectorant mixture, except that it contained very little opium; counter-irritation was produced on chest by means of croton oil. Under this treatment he improved to some extent, but he was removed from the hospital after a few days' stay. Was readmitted on the 20th March with all his symptoms aggravated. More especially there was greater stupor, and a disposition to drowsiness. Similar treatment was prescribed, and besides, a blister was applied to the head. Notwithstanding, comatose symptoms gradually set in, except that his pupils remained equal and natural in size, although passive under the stimulus of strong light. The fatal event speedily ensued.

Post-mortem.—Scalp and calvarium normal, membranes also healthy, except slight opacity of the arachnoid at its upper part, and injection of the pia mater over the posterior part of the right cerebral hemisphere. The brain in the latter situation was slightly softened, but the softening was quite superficial, and did not extend into the white substance. Both white and grey matter, but especially the former, were firmer than usual, and were shrunk and contracted in appearance. There was about half an ounce of serous fluid in ventricles. *Abdomen*—about a quart of serous fluid in the cavity of peritoneum. Liver increased in size, dark yellow and mottled externally. Internally it was of a pale yellow, and had a *fatty* aspect. Weight five pounds. Spleen of average size, but harder than usual. Kidneys rather larger than usual, especially the left one. Externally they were somewhat paler in colour; but in form did not differ from the normal condition. On section, it was found that the cortical substance of both at some points, but particularly the left, was

paler than usual, and partially altered by disease, apparently of a fatty nature. However, this condition was limited in extent, and the principal portions of both kidneys were healthy in aspect.

Remarks.—This case differed considerably from the two preceding ones. In it we have indications of a much fuller development of the drunkard's crisis. There was a diseased state of the integument. The hernial protrusions implied that the muscular system was involved. The liver had undergone a fatty transformation, which, as pointed out by Rokistansky, results from intemperance as well as the small granular one. Although the chest was not opened, we had evidence in the symptoms and physical signs of a blennorrhœa from the mucous membrane of the lungs; and the heart appeared to be hypertrophied. The extent to which the kidneys were diseased was hardly sufficient to account for death by uræmia alone, although the general œdema showed that their functions were decidedly impaired. The indurated state of the brain seemed sufficient of itself to account for the coma, although probably the retention of a part of the urea and other excretory products in the blood from the Bright's disease, aggravated the symptoms and accelerated the fatal result.

These cases, and especially the first two narrated, illustrate the effects produced by a general induration of the brain. The symptoms are just what might be anticipated from so complete an affection of that organ—a gradual impairment of the mental faculties, accompanied by a steadily increasing stupor and paralysis of motion, and deficient sensation, and an eventual subsidence into a partial or complete comatose state. They also unfortunately illustrate the hopelessness of attempting to remove this diseased condition by treatment; although perhaps, along with a removal of the cause, it might probably be productive of more favourable results at an earlier stage of the morbid process.

V.—*Cases of Vesico-uterine Fistula, with an Analysis of a Calculus found in the Os Uteri.* By WILLIAM LEISHMAN, M.D., Physician to the University Lying-in Hospital, Glasgow, Fellow of the Obstetrical Society of London, &c.

VESICO-UTERINE Fistula is, I believe, from whatever cause it may arise, an accident of very rare occurrence. That it may occur, and probably does occur with comparative frequency in those cases of protracted labour, where the vitality of the vesico-

vaginal septum is extensively destroyed, is evident; but here the os is affected, and the vesico-vaginal and vesico-uterine gaps continuous. This has been called by M. Jobert, *vesico-utero-vaginal fistula*. But in vesico-uterine fistula proper, this is not the case. Whether from pressure during parturition, or from some other cause—as in Professor Simpson's cases of pelvic abscess—a communication takes place between bladder and uterus in that limited space in which these viscera are connected together, the result, of course, being incontinence of urine and other distressing symptoms.

It is, at first sight, somewhat remarkable that such a lesion should have escaped the notice of such a careful observer and elaborate pathological writer as Morgagni, and indeed, in so far as we can learn, of all writers on the subject of pathology down to a comparatively recent period. I am not aware, indeed, that until attention was called to vesico-uterine fistula by Madame Lachapelle and by Professor Stoltz of Strasburg, any mention of it whatever is found in medical literature. The details of the following case, which was under my observation for a period of upwards of a year, taken along with the history of a few cases previously recorded will, I think, suffice to afford a reason for this, showing as they do that this lesion, although undoubtedly of a serious nature, is by no means so much to be dreaded as that in which the fistula exists in the vesico-vaginal septum, and consequently not so likely to attract attention; while the obscure nature of the symptoms might easily lead a superficial observer into error. I shall, as illustrating the peculiarity of the case which follows, give a short abstract of the four cases detailed by Madame Lachapelle, M. Stoltz, M. Jobert, and Dr. Simpson.

In Madame Lachapelle's case,* a woman, three days after the loss of the *liquor amnii*, was delivered of a child, after a natural labour. Eight days thereafter, a sudden, involuntary, and continual flow of urine *per vaginam* set in, and a careful examination failed to detect any solution of continuity in the vesico-vaginal wall. After a more careful examination, however, a fistula, four lines in diameter, was found in the neck of the uterus. We have no further history of this case, but are told that it was considered incurable.

Professor Stoltz† gives his case—which ended fatally forty-one days after delivery—at great length. In it, however, the injury was of a more complicated character, as there was a double com-

* "Pratique de l'art des accouchements," Paris, 1825, tome iii. p. 405.

† "Mémoire sur les perforations du col de l'utérus et les fistules vésico-utérine et vésico-abdominale à la suite de l'accouchement." Par M. le docteur Stoltz de Strasbourg.

munication between the bladder and uterus on one side, and between the uterus and peritoneal cavity on the other, the urine passing by this channel into the peritoneum, and causing the fatal result. This can scarcely, however, be considered in the same category as the above.

M. Jobert,* in his able monograph on this and other fistulæ, narrates one case of vesico-uterine fistula, the result of a protracted labour, in which he operated successfully in the following manner. Two lateral incisions were made in the axis of the neck of the uterus, passing quite through the os and reaching to a point above the attachment of the vagina, the mucous membrane of which appears also to have been divided to some extent. By this means the os and cervix assumed the appearance of two flaps, on the separation of which a clear view of the fistula was obtained, and M. Jobert was enabled to reach it and perform the operation by rawing the edges and introducing sutures. The cure was afterwards satisfactorily completed by means of cauterization. In the case of this patient, premature labour was induced by M. Paul Dubois for pelvic deformity, and she was delivered on the 8th of August, 1849. Incontinence of urine having supervened, she was transferred on the 22nd October to the wards under M. Jobert's care, and on the 24th the operation was performed as noted above. On the 22nd of January, 1850, the cure was complete.

Dr. Simpson's case† differs from the others in being the result of pelvic abscess, and not of parturition. It occurred in a patient, aged 22, and mother of two children. Dr. Simpson dilated the os, and touched the exposed fistula with nitrate of silver. "In the course of a few weeks," he says, "the swelling from the deposit between the bladder and cervix uteri diminished, the incontinence of urine became gradually lessened, and was ultimately totally arrested; the cure being, as I believe, the result of the natural contraction of the parts, following upon the absorption of the original inflammatory deposit."

It will be seen from the details of the following case, that it differs from those given above in certain particulars, although, pathologically, the lesion was probably very similar in all.

On Sunday evening, 16th December, 1859, Mrs. D. was attended in her confinement at her own house by two students of the University Lying-in Hospital. She stated that she was 38 years of age, that she had had seven children previously, and that labour had been very protracted on the occasion of each confinement, but that all the children had been born alive and

* "Traité des fistules vésico-utérines, vésico-utéro-vaginales, entéro-vaginales et recto-vaginales." Par A. J. Jobert (de Lamballe), Paris, 1852, p. 39.

† Simpson's *Obstetric Memoirs*, vol. i. p. 232.

without operative interference. The labour pains had commenced on Friday evening, when the *liquor amnii* had come away ; but she had been going about on the Saturday. On Sunday the pains had been frequent and strong, but it was only when she felt that she was making no progress that she sent to the hospital for assistance. Shortly afterwards I was sent for, and found, on examination, that a large coil of the umbilical cord was lying on the bed, cold and pulseless. On introducing two fingers into the vagina I reached the right hand, and, well up towards the brim, the face ; and on introducing the whole hand in order to examine more carefully, I found that the long diameter of face lay between the antero-posterior and left-oblique diameters, the chin resting a little to the right of the symphysis pubis. Posteriorly, to the right side, and a little higher up by the side of the head, was the right foot. The water had evidently been long drained away ; and the uterine contractions being strong and frequent, the face was firmly pressed down, while the child was closely embraced by the uterine parietes.

As it was evident that the pains must be ineffectual in expelling the child, I brought the foot farther down into the vagina, and placed round it a noose of worsted yarn. In the absence of contractions, firm and gentle traction was made by the yarn while the head was pushed upwards, but this was quite unsuccessful until the hand and arm were pushed up beyond the head, upon which the child at once turned and the foot came down. The head was got through the brim with great difficulty, and considerable hæmorrhage ensued.

The child, which was still-born, though not in the slightest degree decomposed, assumed on being laid on its back, and on the legs and pelvis being elevated, the position in which it had been lying *in utero*. The head turned towards the left, the left shoulder was also pushed down, the left arm was lying across the chest, and the left leg across the abdomen, with the toe at the right axilla, and the knee pressing on the umbilical cord. The body of the child was curved towards the right like a bow ; and the right leg, like the cord of the bow, passed over the posterior part of the left side of the head, and the right arm over the middle of the right side of the head. A cast of the child was taken in this position.

On account of the severity of the labour and the subsequent hæmorrhage, the patient remained for some days in a weak state, but was reported on the 1st of January to be slowly recovering.

Jan. 6th—I was called to see her in consequence of incontinence of urine, which had come on a few days after delivery, but which she had not previously mentioned. From her own

account and that of her friends, I was induced to believe that this arose from weakness, and prescribed some remedy which I thought would relieve the symptoms.

22nd—No improvement in the symptoms. The flow of urine was, when she lay on her back, incessant. Lying on her side she could retain it for about ten minutes or a quarter of an hour. On proceeding to examine the parts, I observed the urine escaping by the *ostium vaginae*. I then put her on her knees and introduced Bozeman's speculum, when, the posterior wall of the vagina being elevated by this instrument, I had an opportunity of carefully examining the whole course of the urethra and the vesico-vaginal septum. I need only say here, that a most careful and prolonged examination failed, on two occasions, to detect any lesion. I had instructed her on each occasion to retain some urine, if possible, in order that I might see from whence it came; but she was quite unable to do this, as in the preparatory movements the urine seemed to have escaped to the last drop.

Feb. 4th—On this occasion I examined her in the position in which I found her lying, which happened to be the usual obstetrical position on the left side. I introduced the ordinary tri-valved speculum, with the handle towards the perineum, and, after it was introduced, withdrew the third segment, thus leaving the anterior wall of the vagina exposed. I now desired her to expel what urine was in the bladder, placing at the same time a finger close to the *meatus urinarius*, when I distinctly saw the urine issue by the os uteri in a full stream. Being now convinced as to the nature of the case, and the woman being somewhat exhausted, I left, intending at a subsequent visit to dilate the os and examine more minutely the nature of the fistula.

11th—On visiting to-day I found that she was labouring under a somewhat severe attack of acute bronchitis, which greatly aggravated the symptoms, the urine coming away during the paroxysms of coughing. In these circumstances I thought it better not to interfere. At this time, I may mention, the only case of the kind which I had read in detail was that of Professor Simpson, above noted, in which such fortunate results ensued from a single application of the nitrate of silver. I could not, therefore, but think that, if such was the result in his case, unconnected as it was with parturition, I was justified in hoping that here the natural process of involution of the uterine tissue might prove a valuable auxiliary in the process of cure, although I did not at that time contemplate trusting to this alone.

Not to prolong unnecessarily the details of the case, I may briefly state that I continued to see the case at intervals until December, 1860, when I lost sight of the patient. About six weeks after the date of the last report, there was a marked

improvement in the condition of the patient. The urine no longer flowed incessantly while she lay on her back, and she was sometimes able to retain it as long as twenty minutes, but at the end of that or a shorter period it came away suddenly, and without her control. The period during which she was able to retain her urine now gradually increased, and she ceased to have the slightest discomfort from excoriations. Indeed, it is remarkable in this case how little annoyance she had from this cause from the first. This was due in great part to the scrupulous care which she took in keeping herself dry and clean; but I think it seems also to show how much less severe and distressing this is, than the direct communication between the bladder and vagina.

I saw the case for the last time on the 21st of December, 1860, when I again examined her—this time by means of a trivalved speculum invented by Mr. W. B. Hilliard, instrument-maker to the Royal Infirmary, and described by him along with other instruments for the vesico-vaginal operation, in the *Medical Times and Gazette*, November 14, 1860. By means of this speculum I obtained a perfect view of the parts, and found them in the following condition:—

The mucous membrane of the vagina and covering the os was perfectly healthy in appearance, the os itself being free from the slightest excoriation, and firmly closed. On compressing the meatus as before, and causing her to make an expulsive effort, no urine escaped, while through a catheter passed into the bladder urine flowed abundantly. At this time she was, although still in very delicate health, able to go up and down stairs, and to walk for some distance, retaining her urine with ease for four or five hours. One other point I must not omit to record, although I regret that I had not an opportunity of verifying it.

On asking her if she had menstruated since her last confinement, she said that she had done so regularly for some months, but *only when she made water*. I questioned her more particularly, when she said that at the monthly periods the urine appeared to be mixed with blood. Whether this was menstruation through the bladder, a vicarious discharge, or an accidental hæmorrhage, I do not pretend to say; but I much regret that I had no opportunity of making any further observations on the case, as on my next visit she had left her house, and I was unable to trace her.

Thinking that the above case was worthy of record, I have given it at some length, from notes taken at the time. It certainly appears to me to be a very striking instance of the *vis medicatrix nature*, and serves to show, as I hinted above, how this lesion had been so long overlooked. I am by no means satisfied that the cure was in this case complete, but I think it

will be admitted that the details serve to show that we are justified to expect from nature an amount of assistance in the case of this lesion, which in the case of vesico-vaginal fistula she appears wholly to withhold.

I have been favoured by my friend, Dr. J. G. Wilson, with the details of a case which he met with in his practice some time ago, and which in point of interest is equal to that which I have just narrated, although upon different grounds.

Dr. Wilson was summoned by a midwife, who alone was in attendance on the case, to see a woman who had been for several hours in labour of her fifth child, the midwife being unable to determine the nature of the presentation. On his arrival, Dr. Wilson found the woman much exhausted, with a feeble pulse. On examination he found a sharp and hard body, very irregular in its outline, protruding from the os, which was very slightly dilated. The cervix was quite unobliterated, and its tissues, and those immediately surrounding it, were in a morbidly indurated state. After a short delay, the body filling the os and cervix was seized by means of dressing forceps, and was, with considerable difficulty, withdrawn, along with some adherent portions of mucous membrane. Dr. Wilson had at first been of opinion that it was a fragment of bone, but on examining it after removal, he found that it presented all the characters of a urinary calculus. Finding that the head presented, and that after waiting some hours the os scarcely showed any disposition to dilate, he made a few incisions, which greatly facilitated its dilatation; but the pains ceasing after the head was well down in the cavity, caused him to apply the forceps, by which she was safely delivered.

On making inquiry as to this case, Dr. Wilson found that she had been affected for some months with incontinence of urine, which did not appear to have had any connection with her previous confinement. She stated that she had complete power of retention for a considerable time while standing, but that on assuming the horizontal or recumbent posture, she was quite unable to retain the urine, which came from her involuntarily. Dr. Wilson particularly questioned her with a view to ascertain whether or not she had had a pelvic abscess, but could not elicit any facts which entitled him even to suspect that such had been the case. In fact, the only history he could obtain was, that it had commenced in the course of her pregnancy, some months before the labour came on, but he was quite unable to obtain even a clue to the cause.

Dr. Wilson intended to have examined and thoroughly investigated the case after her convalescence, but he found, on making further inquiries, that she had unfortunately been removed to Ireland, and from that time he never saw or heard of her.

There can, I think, be no doubt as to the nature of this case, although its history is so obscure. The indurated state of the os and cervix is an interesting point in its history, which was most probably the result of the lesion, but may possibly have acted to some extent as its cause, seeing that the accident appears to have taken place after the fifth month of pregnancy, at which time the os would probably be much in the same condition as that in which it was found. I believe that the case is unique as regards the perfect form of the calculus. I have heard of two cases of vesico-uterine fistula, in which there was an irregular deposit of calculus matter in and around the os, but I do not think that any case is on record where the calculus took such a distinct form, with a firm smooth surface. There was not the slightest trace of any other fragments or crust.

As to the calculus itself, and the possibility of its being bone, or containing a nucleus of bone, the following notes of a careful examination and analysis which I made at Dr. Wilson's request, will set that matter at rest:—

The calculus was irregular in outline, bearing some resemblance in form to the foot of an infant, or still more to a miniature shoemaker's last. Its extreme length was, as near as possible, an inch and a quarter, and its extreme breadth seven lines. It was of a greyish-white colour on the surface, and slightly discoloured here and there, with what appeared to be small fragments of dried mucous membrane.

It was of low specific gravity, its exact weight being 44.13 grains.

On making a section of it, I found that it was apparently homogeneous in structure, of a nearly pure white colour, and without the slightest trace of a nucleus, for which I carefully searched. Very minute glistening crystals were seen, but not distinctly without a lens. A small portion of it, when heated on platinum foil, frothed up, gave off slightly ammoniacal fumes, and finally fused into a greyish-coloured bead, which was dissolved in hydrochloric acid in a watch glass. On adding to this an excess of ammonia, precipitation immediately took place, and I found, on microscopical examination, the precipitate to be composed of beautiful stellate crystals and amorphous particles. Another portion was dissolved in nitric acid, evaporated to dryness, and a drop of ammonia added, which developed a faint reddish-purple colour. A third portion was found to effervesce very slightly on the addition of hydrochloric acid after ignition, but not before it. From this rough examination I assumed that it was mainly composed of ammoniaco-magnesian phosphate, and phosphate of lime (fusible calculus), with a distinct trace of uric acid, and probably a little oxalate of lime.

I now proceeded to make a quantitative analysis, the result of which I append. The following is a brief outline of the process adopted:—

1. The moisture was determined by desiccation over sulphuric acid. This I find to be necessary in all cases where the ammoniaco-magnesian phosphate is present, as this substance contains 12 equivalents of water, of which it is said to lose 10 at a temperature of the water-bath (somewhat under 212°). This, however, cannot be depended upon, or it would be easy enough to determine in this manner the quantity of moisture; and I find, therefore, that the only method on which I can depend for accurate results, is by separating the hygroscopic moisture by means of concentrated sulphuric acid.*

2. The total phosphoric acid was determined as follows by the iron process:—The acid solution, when cold, was nearly neutralized with ammonia, an excess of acetate of potash added, and the whole phosphoric acid was then precipitated as phosphate of iron, by means of the perchloride of iron, which was added until the solution retained a brown colour. The mixture was then boiled, filtered, and washed thoroughly for about two hours with boiling water. It was then redissolved in as small a quantity of hydrochloric acid as possible, and tartaric acid added, in order to keep the iron in solution. After it had again cooled, ammonia was added until the precipitate which at first formed was redissolved. The phosphoric acid was now precipitated by the ammoniacal sulphate of magnesia. This, after being washed with a dilute solution of ammonia, was dried, ignited, redissolved, and finally precipitated by ammonia—washed, dried, ignited, and weighed as pyrophosphate of magnesia, from which the total phosphoric acid was easily calculated.

3. The filtrate from the first precipitate of mixed phosphate and acetate of iron, was mixed with oxalate of ammonia and ammonia, and boiled. After leaving it exposed to a gentle heat for twelve hours, it was filtered, and the oxalate of lime washed

* I may mention, as an illustration of this, that I experienced great trouble in this way while analyzing some intestinal concretions from a horse, which are mainly composed of the ammoniaco-magnesian phosphate. I therefore made a few special experiments to determine if the quantity of water of crystallization, lost at 212° , was constant. Three portions of the substance, finely pulverized, were taken from the same bottle, and carefully weighed. They were then put into the water, each at different times, but for the same periods. The following was the result:—

		Loss at 212° Fahr.	
Portion A,	.	36	per cent.
" B,	.	35.501	"
" C,	.	36.224	"
In the air-bath at 240° , the loss was,	.	38.573	"

and cautiously heated to redness in a platinum crucible. The lime was then calculated from the residuary carbonate of lime.

4. The filtrate from the oxalate of lime, which had become from the washings much too bulky, was reduced by evaporation, and the magnesia precipitated in the cold, by means of ammonia and phosphate of soda. This being washed with ammonia-water, as before, was ignited and weighed, yielding, on calculation from the pyrophosphate, the total magnesia.

5. Having had to repeat the above process, I had a very minute portion of the substance left for the determination of the uric acid. This I attempted, as I have done before with success, by dissolving in caustic potash, filtering from the insoluble phosphate, and precipitating by means of hydrochloric acid. The result was, however, from the smallness of the quantity used, so unsatisfactory, that although I state the result here, I do not do so with much confidence in its accuracy.

The following was the composition of the calculus:—

Ammoniaco-magnesian phosphate,	49·84
Phosphate of lime,	28·988
Water,	4·031
Uric acid,	2·487
Mucus and other organic matters (oxalate of lime—a trace—?), alkalies, and loss,	14·654
	<hr/> 100·000

I do not know whether the process which I adopted is in all respects a good one. Probably a practised analyst would adopt some plan both easier and more satisfactory. My only object in giving it here, is in the hope that it may possibly be of use to any one who has such an analysis to perform for the first time, when he will find by repeated failures that it is by no means an easy matter, unless he has some definite plan on which to proceed.

The ammoniaco-magnesian phosphate was calculated from the magnesia found, and the phosphate of lime from the lime. The phosphoric acid required, according to these calculations, agreed with the quantity found by actual determination within ·4 per cent. The quantity of substance was so small that I never thought of a nitrogen determination.

VI.—*Report of a Case of Rupture of the Uterus in the Fourth Month of Utero-Gestation.* By W. B. M'KINLAY, M.D., F.R.C.S.E., &c.

(Read before the Glasgow Medico-Chirurgical Society.)

ON the evening of Sabbath, 31st March, I was requested by Mr. Macfarlane, one of the procurators fiscal of Renfrewshire, to proceed at once to a village distant about eight miles from Paisley, for the purpose of making a post-mortem examination in a case which seemed to be of a very suspicious character. The circumstances were the following:—An unmarried female who had been previously in a good state of health, and had been out the evening before, had died in a neighbour's house, or rather in a house where she slept at night, and none of her friends had been made aware of her illness until after her decease. She had gone to bed about eleven o'clock the previous night, not complaining, but rather in better spirits than usual, and was dead about seven o'clock the following morning.

On arriving at the place, along with Dr. Richmond of Paisley, whom I associated with me on the occasion, I learned the following particulars:—The deceased, B. T., was aged about 37 years, of a robust habit of body, and had always been healthy. Was a farm-servant, but had latterly come into the village for the purpose of taking care of her father, who was an infirm old man. On the day previous to her decease, she had gone by railway to a neighbouring village about four miles distant, accompanied by Mrs. W., in whose house she slept. They returned on foot, and called at a farm-house, where they partook heartily of tea, to which they had cheese. During the whole of that time the deceased was in good spirits; indeed, those who saw her, state that they had never seen her more cheerful. On their return home she remained in the house for some time, and between half-past eight and ten o'clock went out with a daughter of Mrs. W. and visited some friends. When they returned, she went into her father's house, prepared his supper, milked his cow, and returned to Mrs. W.'s at about a quarter to eleven o'clock, shortly after which time she retired to bed, not complaining, and seemingly in good health and spirits.

Shortly after going up stairs to bed, a little girl came down and stated that the deceased was very ill, on which she was seen by Mrs. W. She stated that she was much pained in the stomach and bowels. When asked if she thought she had walked too far, she answered not; she said, however, in answer to a question put to her, that she was afraid of inflammation. At this time she was vomiting. A medical man who was in the

house was asked to see her, with which he complied. He examined the vomitings, and having found some portions of the cheese of which she had partaken to tea, he said it was only disorder of the stomach, and ordered a mustard blister to be applied over the stomach. She applied the mustard herself, and stated that she felt relieved by it, after which she lay quiet for some time. About half-past one o'clock she became worse, and the mustard was a second time applied by herself. When asked at this time if she was pregnant, she would give no answer. Between two and three o'clock a dose of castor oil was administered to her, which she seemed reluctant to take, and on being asked the reason, she said that feeling her stomach a little out of order, she had taken a teaspoonful of sulphur in her father's house before she came in. This would be between ten and eleven o'clock. The mustard was applied a third time, but no alleviation of the symptoms took place. About four o'clock she was said to have confessed in an ambiguous manner that she was pregnant. Shortly after this she became quiet, and died about a quarter before seven o'clock. No other person was called in, nor, as before stated, were her friends, who lived next door, made aware that anything was the matter with her. Suspicion subsequently fell upon Mrs. W. and another.

COPY OF REPORT.

"PAISLEY, 1st April, 1861.

"This morning, within the house occupied by Mrs. W. at C—, parish of L—, and shire of R—, we, the undersigned, made a post-mortem examination of the body of B. T., aged 37 years, daughter of W. T., gardener, residing at L—.

"On the lower part of the cheeks and around the mouth, there were several reddish spots, but there was no mark of violence on any part of the surface of the body. Over its whole extent, the skin was very much paler than usual. Both pupils were dilated to a considerable extent; the teeth were slightly apart, and the tongue was not protruding. On proceeding to lay open the various cavities of the body, the cellular tissue covering them was found to be more abundant than usual. On laying open the chest, the lungs were found perfectly normal in appearance, and at no part of their surface was there the slightest adhesion to the pleura costalis. There was no effusion into either cavity of the pleura. The pericardium contained about two fluid drachms of serous fluid. The heart was flaccid, and the right ventricle contained a small quantity of dark fluid blood. The right auricle did so likewise. The left ventricle also contained a very small

quantity of blood of a similar description. The left auricle was empty. All the valves of the heart were normal.

"On laying open the abdomen, the cavity was found to contain a very large quantity of serum, blood, and clots of blood, which, upon being removed, were found nearly to fill a large wash-hand basin. The stomach, which was very much contracted, was carefully secured by double ligature, and removed from the body. Upon being opened it was found to contain a small quantity of a whitish pultaceous mass, having bits of what appeared to be cheese, and also a number of small blackish particles mixed up with it. The lining membrane was in several places much reddened. The whole stomach and its contents were carefully placed in a clean glass jar which was duly sealed, labelled, and preserved for further examination. On laying open the œsophagus and pharynx above the ligature which had been placed upon it, they were found to contain a small quantity of fluid through which were floating some globules of oil; part of this was removed and carefully preserved. The liver was normal, both with regard to size and appearance. The pancreas, spleen, and kidneys, were of a natural appearance. The duodenum with its contents were removed for further examination. The rest of the bowels were generally pale in colour, and at no part was there the slightest appearance of inflammation.

"Projecting from the pelvis, there was a large tumour covered with clotted blood. This upon careful examination was found to be the uterus, partially protruding, through a rent in the fundus of which there was found a foetus apparently about the fourth month of utero-gestation, covered with its proper membranes. The foetus was carefully removed, without dividing anything but the membranes and the umbilical cord, when the rent was found to be entirely across the fundus, and measured transversely four and a quarter inches, and in an antero-posterior direction it measured three and a half inches. The uterus at the rent was normal in structure, and at no part was there any appearance of disease. Part of the pubis and ischia were carefully removed and the organs of generation dissected from the labia inwards, but not the slightest mark of injury or discoloration could be perceived, and at the os uteri and in the neck there was found a plug of clear mucus completely closing the entrance to that organ. The distance from the os uteri was six and a half inches. The bladder contained about an ounce of whitish urine. On removing the calvarium the vessels of the brain and its membranes were found slightly congested. The substance of the brain was normal.

"We are of opinion that the death of the before designed B. T. was the result of rupture of the uterus and consequent effusion

of blood into the cavity of the abdomen ; and, taking into consideration the healthy state of all the viscera and the absence of any mark of irritation except in the stomach ; we consider further that such a rupture in all probability was caused by the action of some emenagogue or uterine excitant taken into the stomach and system, which can only be ascertained by a chemical analysis of the parts preserved.

“ This we certify on soul and conscience,

“ W. B. M'KINLAY, M.D., F.R.C.S.E.

“ D. RICHMOND, M.D.”

In accordance with an order from Crown Counsel the whole of the substances removed at the examination were handed to Professor Penny for the purpose of being submitted to analysis ; but the result was entirely negative, as no trace could be found either of ergot, savine, or other noxious substance.

I have brought the before detailed case under the notice of the society in consideration of the rarity of the occurrence, and the peculiarities of the case itself. Here, a very well-formed woman, in a perfect state of health, without using any exertion and apparently without receiving any personal injury, in the fourth month of pregnancy, becomes ill at eleven o'clock at night, is seized with severe pain in the stomach and bowels which, although, according to Mrs. W. who attended her (and her account must be received with great caution as she was considered implicated, and indeed was taken up on suspicion of using or assisting in using some foul play towards the deceased), it was somewhat relieved by the application of sinapisms, yet the pain more or less increased till four in the morning when it ceased. The medical man who saw deceased, gave a very confused and somewhat contradictory account of the progress of the case. On our examination there was not the very slightest mark or appearance of mustard or anything else having been applied, nor was there even the slightest scratch upon the skin, and most assuredly there was not the least malformation in any part. The only corroboration of their statements was the oil in the pharynx and œsophagus. With regard to the opinion we formed on examination, I consider we are fairly borne out. The only organ in the body in which there was the least appearance of irritation was the stomach, which was found very much contracted, and in several parts the lining membrane was very much reddened, and throughout the whitish pultaceous contents there was a number of small blackish particles. These were examined at the time, though somewhat imperfectly, by a small pocket microscope which I had with me, and were considered by Dr.

Richmond and myself somewhat to resemble ergot; however, on account of the stomach and contents being preserved for further examination, we did not examine so minutely as we would otherwise have done.

Again, let us look at the state of the uterus itself. The whole of the abdominal cavity was filled with serum, blood, and coagula; and covering the uterus, the clots were very thick, requiring great care in their removal in order not to displace any part of the organ, so as to get it examined in a normal position. When the clots were removed, as stated in the report, the fœtus was found partially projecting through the rent. The whole of the uterine structure had been torn to the extent stated, and after careful examination, both by the naked eye and the microscope, no change could be detected, nor did the organ in any part bear the least appearance of thinning or disease. With regard to the fœtus itself, it was entirely invested in its proper membranes which did not appear to be much if at all disturbed. The only other means through which I consider such an occurrence could take place, would be by pressure on the abdomen with some soft substance intervening.

I have looked into a number of works on the subject, and the only work in which I can find anything to the point is Burns' Midwifery, or rather in notes attached to the work, where he gives a case described by Dr. Drake, in which the uterus seemed to burst at the fourth month, producing suppuration at the umbilicus, where an opening was formed by which excrementitious matter was discharged for some time.

Again in the *Journal de Médecine*, 1780, there is the case of a woman who had the uterus ruptured in the fourth month of pregnancy. The accident was followed by uterine hæmorrhage, which continued for some time. The menses returned, but the body did not subside. In the ninth month she died. The uterus was found of the natural size, but the rent was still perceptible.

Merriman, after stating that rupture of the uterus has happened from a morbid state of that organ before the period of utero-gestation has been completed, directs attention to a case detailed in the seventh volume of Medical Reports, by Mr. Ilot of Bromley, where rupture of the uterus took place in the sixth month of pregnancy. The patient was awakened from her sleep by a sudden pain about the umbilicus. She had no return of pain, but sank and died. The fundus uteri was found ruptured.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

I.—*On Supporting the Perineum.* By GRAILY HEWITT, M.D., Lond., M.R.C.P.; Physician to the British Lying-in Hospital; Lecturer on Midwifery and Diseases of Women and Children in St. Mary's Hospital Medical School, &c. Pp. 70. London: John Churchill, 1861.

A SERIES of papers which appeared in the *British Medical Journal* under the above title, and which are now reprinted by Dr. Hewitt in a neat pamphlet form, attracted, at the hands of the profession in London and elsewhere, no inconsiderable amount of notice. For this we find on a perusal of the book a very obvious reason—exclusive of the intrinsic merits of the work itself as a piece of close logical reasoning—in the fact that Dr. Hewitt seriously questions the propriety of that extremely tiresome operation, familiarly known to all obstetric practitioners as “supporting the perineum.” On what grounds he does so we shall endeavour to show by an analysis of his memoir, premising merely that, although differing in minor points of detail, he agrees essentially in his conclusions with the author of a paper in the *Glasgow Medical Journal* for January, 1860, to which he frequently refers.

The main object of the author is to consider thoroughly, and in a practical manner, the causes and prevention of laceration of the perineum during labour. He opens his case as follows:—

“The fact with which we have to deal is that, in a certain proportion of cases, the birth of the child is the occasion of an accident to the perineum: the soft parts which surround the inferior termination of the parturient canal are lacerated by the mere mechanical act of stretching which they undergo; in other words, laceration of the perineum takes place.

“With the knowledge of this fact before them, practitioners have been accustomed to look on laceration of the perineum as an accident always liable to occur; and a certain preventive procedure, known as ‘supporting the perineum,’ has been invented. This has been adopted by almost all the highest obstetric authorities, systematically enjoined by obstetric teachers on their pupils, and most extensively carried into effect by practitioners throughout, it may perhaps without exaggeration be stated, the civilized world. It is only quite recently that the propriety of the practice alluded to has been at all seriously questioned; and at the present time the almost universal practice still is to endeavour to prevent perineal laceration by, as it is termed, ‘supporting the perineum.’

“Is the treatment in question proper? Is it based on sound principles? Is ‘supporting the perineum’ really and truly calculated to prevent laceration of this part during labour? If these questions are to be answered in the negative, much obstetric teaching that has been quietly acquiesced in for many years

past will have to be thrown aside; and it will be rendered evident that much valuable time and very much physical exertion have been expended, that much anxious thought has been bestowed, on what was, after all, valueless and to no purpose. If the thing be good, let it be retained; if it be bad, let it be discarded."

Dr. Hewitt takes exception, in the first place, to the word "support," as applied to the perineum, which, in the present state of obstetrical knowledge, has no fixed meaning, and does not admit of accurate definition, from the fact that scarcely any two authors are agreed as to its signification, and still less as to the object to be attained by the practice.

What then can a student do but accept the *ipse dixit* of his teacher, or of the author of his midwifery text-book? If, however, he should be of an inquiring turn of mind, and pursue the matter a little further by comparing the works of some of the most eminent obstetrical authorities, in order to have a clear idea of the *rationale* of the process—what, we would ask, would be the effect on his mind of such notes as the following?—

"When the perineum begins to swell, it must be firmly supported by the palm of the hand to press against it in the time of a pain. For this purpose the hand should be applied in such a manner as not only to give a suitable support to the perineum, but, as the head advances, to regulate its progress by pressing the perineum, as it were, backward in a direction towards the coccyx."—*Hamilton*.

"By drawing it backwards and upwards over the protruding parts, rupture may sometimes be avoided."—*Dr. Snow Beck*.

"Press the head forward towards the symphysis pubis, and prevent the whole force of the uterine action being directed against the perineum."—*Dr. Lee*.

"Our object is not merely to support the perineum, but to direct the head as much forwards under the pubic arch as possible, in order that the anterior portions of the os externum should undergo their share of dilatation, and thus in some measure spare the perineum." The object of so placing the hand is to "push the soft parts somewhat forwards, and thus relax them. By this means we not only direct the head against the other parts of the os externum, but avoid the danger of its perforating the perineum."—*Dr. Rigby*.

"Pressure on the perineum acts injuriously by inducing reflex contraction of the uterus."—*Dr. Tyler Smith*.

"The object of supporting the perineum appears to me to be twofold:—First, to prevent or allay irritation, and to diminish congestion, so that the act of dilatation may not be interfered with. Secondly, to counteract too violent action of the uterus."—*Dr. Murphy*.

"Place your elbow against the bedstead, regarding it as a fixed point, and allow the perineum to be forced against your hand."—*Dr. Ramsbotham*.

"Offer some gentle support externally against the pressure internally, but do not retard the progress of the child."—*Dr. Fleetwood Churchill*.

"Under any circumstances any support of the perineum is unnecessary."—*Nägele*.

It is, we think, more than probable that the hypothetical searcher after truth, whom we suppose to have made these notes for his guidance, would not be greatly benefited thereby. Nor is it, we imagine, much more likely that any one who, like Dr. Hewitt, takes the trouble to study the matter thoroughly, will by means of such study attain anything like a satisfactory knowledge on the subject, either as regards what is meant by "support," or the *rationale* of the process. It is a natural result of such an investigation that we should seriously ask ourselves whether or not this, on which opinions differ so widely, is a practice which is necessary in every case, or even advisable as a plan for general adoption. And here we must not forget that the practice in question is comparatively a modern one; indeed, so far as we know, no mention of it is made in any work on midwifery until 1781, when Professor Hamilton of Edinburgh published his "Treatise on Midwifery," in which elaborate directions are given for the support of the perineum, by which operation, he says, "the miserable consequences will be prevented, to which the neglect of the perineum may expose the woman."

Some early writers go so far as to say, "that by proper management of the second stage of labour, perineal laceration may invariably be prevented,"* but we are quite sure that none of the eminent obstetrical authorities of the present day would endeavour to maintain this for a moment. Indeed it is admitted that in spite of support, and every possible precaution, laceration does sometimes occur. This entitles us to assume as a fact universally admitted, that support of the perineum does not *insure* its safety, so that the only question now to be answered is—Does it in any way contribute to its safety by averting the danger of rupture?

Dr. Hewitt's little work is in substance an attempt to answer this question, which, after elaborate reasoning and careful study, he does in the negative. He considers, in the first place, the effect which supporting the perineum may produce on the advancing head. In discussing Dr. Tyler Smith's theory of reflex contraction of the uterus being caused by pressure on the

* *Osborne—Essay on the Practice of Midwifery.* p. 28. London, 1795.

perineum, a fact which had previously been noted by Madame Lachapelle, he gives a theory of his own, which merits attention, as being simple as well as original. He says:—

“The explanation given by Dr. Tyler Smith of this fact does not appear to me to include the whole truth. Is not the acceleration due rather to the effect of the resistance offered to the advance of the head of the child, than to the excitation of reflex action? Any resistance to the action of the uterus calls that action into more violent and powerful manifestation, as is well known; and I should be disposed to explain the beneficial ‘accelerating’ action of pressure on the perineum in this manner. I am, at the same time, quite prepared to admit that reflex contraction of the uterus may be set up by continual pressure on, or by irritation and manipulation of, the perineum. All I contend for is, that this is only a part of the explanation.”

Dr. Hewitt considers that pressure, however applied, is quite useless if employed with a view to direct the head forwards under the pubic arch, but in this we cannot agree with him, convinced, as we are, that firm pressure on the sacrum and coccyx, as recommended by Dr. Rigby, is highly efficacious in aiding the last pains. We believe, however, that the good effects of this mode of procedure do not result directly as an effect of the pressure, but that they arise more from the manner in which the head is thus prevented from retreating during the intervals between the last expulsive efforts. On this point the author proceeds to make the following remarks:—

“There is an effect which might be produced on the head of the child by this attempt to ‘direct the head forward’ in supporting the perineum, which has not been claimed for it by the adherents of the practice in question; namely, the possible diminution of that diameter of the foetal head presented to the perineum, by the pressure exercised from without. If the head be, as it ordinarily is, compressible, it is clear that pressure from behind would, if sufficiently strong, have a tendency to lessen that diameter of the head which lies between the hand of the operator and the resisting arch of the pubes. In fact, as in the case of the application of the forceps, it might be said, here you have a means of rendering the passage of the head more easy for the perineum; and I can quite conceive that pressure might be so directed by the hand as to actually produce a tangible effect of this kind. The obvious criticism on such a mode of facilitating the delivery would be, that in the use of the forceps there is a better means of arriving at the same end. Compression by the forceps would at the same time be more effectual and less liable to injure the soft structures, which, in the other case are necessarily interposed between the head of the child and the hand of the operator—the perineum.”

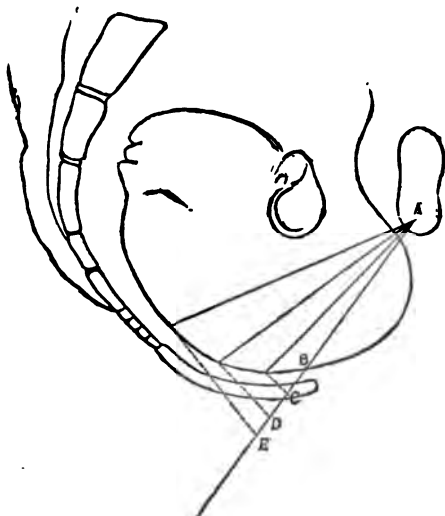
Proceeding now to consider the effect of pressure on the perineum itself, Dr. Hewitt minutely considers the method in which nature effects the dilatation of its various structures during normal labour. He shows how pressure, in the first place, prevents equable dilatation of the perineum itself, and how pressure forwards of the fourchette has only the effect of lengthening the canal through which the child has to pass. “What would be

thought," says he, "if an individual who, desirous of enlarging the aperture of a bag, set about the accomplishment of his purpose by endeavouring to lengthen the bag itself?" He shows further, by an experiment in which a portion of the area of an elastic tissue is confined and prevented from stretching with the rest of the substance, an interesting illustration of the effect of pressure on the perineal structure.

The effect of supporting the perineum in delaying, impeding, or interfering with the normal process of distension and dilatation of the perineum is what our author now discusses, and in this section of the work lies the pith of his argument. The following are his remarks on this point:—

"The manner in which the dilatation of the perineal structures takes place, its direction and nature, have been alluded to; and the errors of prevailing notions regarding the same have been pointed out. But the *agency* of the dilatation in question—the force by means of which the perineal surface, previously so small, is increased and extended so as to allow the head of the child to pass—have not been considered; nor whether the plan of 'supporting the perineum' is calculated to modify, assist, or interfere with this important process.

"In 'making its sweep,' as it is termed, over the perineum, the portion of the head encircled by the ostium vaginae progressively increases in circumference;



the perineum has, on every successive pain, to encounter and deal with a section of the head becoming larger and larger. Thus, whereas at the commencement of dilatation the diameter presented to the perineum is roughly represented in the accompanying engraving by the line A B, as the head advances the diameter presented to and stretching the perineum is represented by the line A C; and, before the head can be delivered, the ostium vaginae has

to be dilated to an extent represented by the lines *A D* and *A E*. The real effect is here a little exaggerated, owing to the circumstance that the point *A* is not a fixed point, this part of the occipital region also advancing, although very slowly, beneath the pubic arch. The head is thus brought into contact with the perineum in such a manner as to produce dilatation of its structures in all directions. Just as every portion of the superficies of a hollow india-rubber ball is acted upon and contributes its quota to the distension produced on forcibly blowing air into it, so is every portion of the perineal surface stretched and made to contribute to the extension of surface necessary.

"The process by which complete dilatation is arrived at, and which, like many others in the animal economy, is calculated in every way to excite our admiration, is generally a very gradual one, especially in a first labour. The head descends, driven downwards by the uterine contractions; and the attendant receives an intimation that the perineum is about to undergo dilatation, in the fact that the perineum is slightly bulged downwards during the acme of the uterine contraction. When the pain has gone off, the perineal tumour has entirely vanished also. The next pain propels the perineum downwards a little more; but the difference between the effect of two successive pains is so slight that it is hardly perceivable. To this process of alternate stretching and repose the perineal structures are subjected for a period varying according to the peculiarities of the case—perhaps, also, according to the treatment pursued by the attendant; and at last the vaginal outlet is distended so as to allow the head to pass. The extreme distension of the perineum never, normally, lasts for more than a very brief period; and, even up to the very last, alternates with a state of almost complete repose. The attendant watches anxiously, thinking each pain will be the last; but no; at the moment when the head threatens to escape, the pain suddenly ceases, the head retires, leaving the perineum apparently undistended, and the repose may be so complete that the patient actually often falls asleep. The very next pain may propel the head through the outlet.

"Such is the normal process, which appears to be one of alternate attack and retreat, the attack not being more vigorous than the retreat is decided. The construction of the perineum is such that this peculiar method of dilating its structures is the best and the most efficacious. It appears to be owing to the elasticity of these structures that the head is driven backwards into the pelvis when the uterine action ceases; and without doubt this is nature's provision for safety.

"Now, what takes place when the perineum is 'supported' persistently and forcibly, and in such a way as recommended by certain authorities? If the uterus be acting feebly, such support may render the attempts on the part of nature to dilate the perineal structures futile, or may so impede the action of these efforts as to postpone the delivery for a considerable time. In the case of a 'rigid perineum,' as it is called, what we require is that it should be distended. How is this to be accomplished, if the means nature has provided to this end are rendered nugatory? How can the structures be properly stretched, if every uterine contraction is opposed by the operator's hand forcibly pressed against the perineum? If the uterine pains be violent, they will, in spite of the operator, drive the head downwards; if they are weak, the labour will be rendered tedious by the interference of the 'support.'

"The majority of authorities do not recommend the 'support' to be carried to such a degree as to interfere materially with the distensive action of the head on the perineal structures; and so far as this part of the question is concerned, the slight support they recommend probably does very little harm or good. When the 'support' is in degree such that the formation of the perineal tumour is actually interfered with, the labour is not merely rendered tedious. The support assiduously rendered, has, we will suppose, prevented any great

distension of the perineum, and at the last, consequently, when the head emerges, it is distended to this extent for *the first time*; whereas, in the absence of assistance, the same degree of dilatation—practically the same, at least—would have been produced three, four, or more times, in as many pains, before the final expulsion of the head."

The causes of laceration of the perineum during labour are now enumerated and treated of *seriatim*. The first point here noticed is the comparative frequency of the occurrence of this accident in primiparæ; and the statistics of Dr. Snow Beck and Mr. Baker Brown are cited as striking proofs of this. This, however, is a fact so universally admitted, that it requires no more than a passing notice. From the statistics of these gentlemen, as well as from numerous other sources, he deduces the fact which few if any will deny, that the forceps, especially in unskilful hands, is a fertile source of rupture. Dr. Hewitt considers, however, that were we to look for accurate information concerning the causes of laceration, we would on the whole get very little satisfaction from the perusal or multiplication of statistics, and he therefore proceeds to inquire what are the deductions obtainable from reasoning.

An undue force in the uterine contractions Dr. Hewitt concludes to be the cause of laceration in a large proportion of cases, as it may be fairly assumed that here the perineal structures would be too suddenly and too roughly dilated to be consistent with safety to the perineum. This argument appears to us to be that on which the advocates of perineal support could, with most reason, ground their conclusions; although it is not, so far as we are aware, claimed by them as such. The reason for this is evident, as it would only justify them in supporting the perineum in these cases where the uterine contractions are excessive; but so far from warranting them in supporting in natural labour would seem to furnish an *a priori* argument against it, by involving the admission of *excessive* uterine action only, as a cause of rupture.

Dr. Hewitt now touches briefly upon the share which is taken in the production of this accident by abnormal conditions in the presenting part of the fœtus, and by deformities of the pelvis of the mother, and then proceeds to consider what are the conditions of the perineum itself which conduce to its laceration. Here he objects to the term "rigidity of the perineum," but we think, with all deference to him, somewhat unnecessarily. It is all very true that the rigidity here spoken of is simply, as Dr. Hewitt says, a persistence of its normal dense, thick, and resistant condition, and, therefore, should not be termed "rigidity," if we adhere to its true physiological meaning. This we willingly grant, but at the same time we must say that to us the term has

a simple unmistakable meaning, in expressing that the structures of the perineum offer an undue opposition to the dilating force. To look for logical accuracy in professional terms which have been long in use, is more than we are entitled to do, unless we are prepared to substitute a better term, which would be, for obvious reasons, not a very easy matter in this case. The author is, however, quite right in stating that "the term 'rigidity' ought evidently to be restricted to those cases in which the head of the child, having had time and opportunity, so to speak, for producing an effect on the perineum, has failed to do so." Cazeaux says that in rigidity of the perineum, the muscles are thrown into spasmodic contraction, but of this there is, we think, no evidence whatever.

Congestion and inflammation of the perineum is alluded to by Dr. Murphy, as likely to interfere with its dilatation, and it is in order to prevent the effects of these actions that he recommends perineal support. We quite agree with Dr. Hewitt in considering that there is not the slightest foundation for such an assertion; indeed we are astonished that a man with the great experience and undoubted talent of Dr. Murphy, should broach such an idea, which appears to us to be untenable even on theoretical grounds. Under this head Dr. Hewitt mentions congenital smallness of the vaginal outlet, artificial narrowing of the same, and fatty degeneration, as causes of rupture arising from the state of the maternal parts. With reference to the last he says that there is no evidence, and aptly remarks that laceration occurs more frequently in primiparæ, in whom the perineum might be reasonably expected to be particularly sound. We here agree with Dr. Hewitt that we have no right to assume this as a cause, except perhaps hypothetically as a possibility.

The extraneous causes of rupture are now stated:—1st. Those cases in which the thighs are kept spasmodically closed, or in which the head, from any other cause, is prevented from passing forward beneath the pubes. 2nd. The artificial support given to the perineum. This, which was first stated as a cause of rupture of the perineum in the paper in the *Glasgow Medical Journal* above alluded to, is, to a certain extent, admitted by Dr. Hewitt as such. He does not think that, practically speaking, many cases of laceration do proceed from this cause, but is of opinion that the practice recommended by Dr. Ramsbotham would, if carried out universally, greatly increase the number of cases of ruptured perineum. 3rd. The use of instruments. These, especially in the hands of the unskilful or the inexperienced, are but too certainly a cause of this accident.

With reference to the treatment of the perineum during labour, Dr. Hewitt thinks that it should be let alone. Nature in the

vast majority of cases will effect dilatation surely and safely, and needs no assistance from the hand of the accoucheur; but when the pains are severe, and the head advancing with too great rapidity, the hand, Dr. Hewitt thinks, should be placed directly against the head itself, in order to check its onward progress, without interfering with the perineum.

Dr. Hewitt, concludes as follows:—

“The case which I have, in the course of the series of practical observation now brought to a conclusion, argued, is the one of rational *versus* empirical treatment of the perineum during labour. I think there can be little doubt as to the nature of the decision which will be formed by those who take the pains to consider the question attentively and dispassionately.”

We certainly think with Dr. Hewitt that a decision is easy when one takes the trouble to study the matter thoroughly. That support of the perineum is, as recommended by Dr. Ramsbotham, both preposterous in theory and hurtful in practice, we fully admit, and we are all but persuaded that in every case it is practically useless. We think, however, that there are very few judicious practitioners who follow Dr. Ramsbotham's advice to the letter; indeed, our experience goes to show that by far the greater number of those who think it necessary to support the perineum, do it in the gentle way recommended by Dr. Churchill, which if it does no good, can, we think, do no possible harm.

We are inclined to agree with a contemporary who, in reviewing Dr. Hewitt's work, remarks that the author will find it no easy matter to uproot a practice so long established and so generally adopted. Among other reasons for this, we find the fact that women who have had children before, generally expect that something should be done for their *assistance*, and would think their medical attendant, in many cases, guilty of neglect and carelessness, if he did not afford them the same devoted attention to which they had previously been accustomed. This, to be sure, is far from being a scientific reason, but it is not the less a true one. We think Dr. Hewitt may be well contented if he can impress the professional mind so far as to cause that mischievous practice which Dr. Ramsbotham and certain continental authorities advocate, to be discontinued. So long as practitioners follow Dr. Churchill's advice, it is but a question of wasted time and misplaced exertion at the most. No bad effect can, we think, possibly ensue. Still, even to those who adopt this mode of practice, we would recommend a careful re-examination of the whole matter. The subject is interesting enough in itself to repay him who chooses to enter upon a study of it, especially as the literature of obstetrics abundantly proves that very little

attention has been given to the *rationale* of the process by most systematic writers.

With regard to Dr. Hewitt's little work, we have only now to add that we have perused it with great pleasure. His subject is treated not only in a manner able and comprehensive, but with a courteous regard to the opinions of others with whom the author may differ, which is too often lost sight of in publications of a controversial nature. The book itself is certainly the best and most complete hitherto published on this subject. It is carefully and ably written, and is evidently the result of much study and experience. Several wood-cuts, illustrating the text, are introduced with good effect, one of which we have given as tending to explain a passage which would be otherwise unintelligible. The work does its author much credit, and we shall always look with interest in future to anything from the same pen.

II.—*A Treatise on the Surgical Diseases of the Eye.* By H. HAYNES WALTON, F.R.C.S., &c. London: John Churchill, 1860. 8vo, pp. 686.

A VERY common complaint against practical treatises on medical, as well as on other subjects, is, that they give elaborate explanations of matters which a reader of average information and intelligence could explain perfectly well for himself, while they leave him in the dark with regard to the very points on which he requires instruction; and that they give directions, confusing, it may be, from their very prolixity, and at the same time couched in such general terms as to be of little practical avail.

We want to know what to do in a particular case, and how to do it; and we are treated to a mélange of conflicting opinions and practices, vaguely expressed, or rather hinted at; while, for the execution of details, we are referred to general principles and proverbial wise saws. We wish to be aware of the dangers and accidents which are most likely to befall us unexpectedly, and of the means of averting them; and we are advised to be both bold and prudent, we are admonished of risks which any one might foresee, we are cautioned against practices which no one in his senses would adopt, we are warned of possible catastrophes, darkly indicated but too tremendous for explicit narration, and we are threatened with professional ruin and the stings of a guilty conscience if any such calamities should happen in our hands.

These faults are most provoking to the reader when they are met with in works bearing the character of manuals or text-books intended for consultation in cases of emergency, or for the rudimentary instruction of those whom circumstances have precluded from much practical acquaintance with the special topics discussed in them; and, perhaps, they occur more frequently in books of this class than in those of any other, for these are too often the productions of authors who have a greater familiarity with the art of writing for the book-market than with the arts of which they profess to treat, and who derive their inspirations, partly from the labours of their predecessors in the same walk of literature, and partly from a hurried review of the contents of some standard work, giving the finishing touch to their performances by the introduction of a few novelties from the periodicals, in order to bring them up to "the present state of the science."

Hence, too, the unfortunate student or young practitioner is apt to acquire very false notions of the relative importance of different branches of a subject, and of different principles and procedures connected with it, and is often found laboriously storing his memory with strange traditional maxims which have originated nobody knows how—most probably in the misapprehension of somebody's crotchets—which no man of practical experience pays the slightest attention to, and which no one has ever attempted to refute, either because their antiquity is venerable, or because of an apprehension that they may have their rationale after all, grounded perhaps in the eternal fitness of things, and consequently defying all human analysis.

In the perusal of another description of works possessed of more solid merits, the inexperienced reader is liable to be disappointed by finding that his author is writing for a class more advanced in a knowledge of the literature of the subject and in practical experience than he is; and he is perplexed by technicalities with which he is not familiar, and wearied with controversial discussions and practical deductions of which he is not in a position to apprehend the bearings. What he wants is a concise statement of facts and exposition of principles, with plain directions for their application. He wishes to know what an author, in whose professional attainments he has confidence, would think of such diseases, and do in such cases as those which he has to treat; and if he can find this out, he does not much care to be informed of all the pathological theories and hypotheses which have been advanced, and of all the therapeutical proceedings which have been suggested in regard to them.

To all such readers who may have occasion to seek for information on matters of ophthalmic surgery, we would recommend Mr. Walton's "*Treatise on the Surgical Diseases of the Eye*;"

and we have been thus lengthy in the introductory part of this notice, because one of the great excellencies of this book consists in its being less obnoxious to any of the objections indicated above than most works of its kind.

The reader will find in Mr. Walton's book a plain and succinct account of each diseased condition which is brought under consideration, with a reference to such anatomical and pathological questions as relate especially to its operative treatment. The directions for operating are generally clear and judicious, and not too complicated; and they are accompanied by descriptions and wood-cuts of the principal instruments recommended by the author. Mr. Walton appears to have aimed at presenting a view of his own matured opinions, and of those modes of operating which he considers the most eligible, rather than at furnishing the profession with a cyclopædia of all the notions and practices which have found favour with one or other of a host of authorities, ancient and modern. At the same time, he by no means confines himself to this, and he gives us a fair statement of such views of others as he considers of historical and practical importance, even when they do not happen to coincide with his own, evincing, in this respect, a pretty extensive acquaintance with the literature of the subject. In the discussion of disputed questions, Mr. Walton generally states his views decidedly, but so modestly and temperately that, if we cannot always agree with him, we are always constrained to treat his opinions with respect.

We must remark in addition, that Mr. Walton's style is agreeable; and that, without having recourse to anything like gossip or flippancy, he has succeeded in producing a volume much more entertaining than similar works usually are.

In making these remarks, we do not wish to be understood as recommending the book to the tyro alone. Its object is of a more ambitious character; and we believe that the oculist of some standing may derive much profit from its perusal; for it contains many reflections and suggestions calculated to call up the reminiscence of forgotten experiences, to point their moral, and to enhance their practical value. For a treatise on a special branch of a speciality, it is a pretty large one; and there is therefore ample room in its pages for entering fully upon many of the less obvious, but not least important practical considerations, connected with the subject. To treat these satisfactorily, a man must both be conversant with the principles and practice of general surgery, and possessed of opportunities, both extensive and prolonged, for observation in his own particular department; and, in this respect, we regard Mr. Walton's work, like others we could name, as an important testimony to the value of the

much-abused special hospitals, when attended by men who are not themselves mere specialists.

The first edition of this work appeared several years ago under the title of "*A Treatise on Operative Ophthalmic Surgery*;" but as Mr. Walton is not above availing himself of the suggestions of his critics, he has changed it to one more in accordance with the scope of the book, and has moreover omitted, altered, and added so much, out of regard to their opinions and his own sense of the requirements of the profession, that it may be looked upon as completely remodelled; and we think it has been decidedly improved.

In the first chapter we have some remarks on the use of chloroform in ophthalmic surgery. The precautions usually enjoined are well set forth; and Mr. Walton justly lays much stress on the necessity of inducing complete anæsthesia before the commencement of operations. This is peculiarly desirable in operations on the eye, because it is almost impossible to continue the administration of the drug satisfactorily during their performance; while the temporary interruption of some operations on the internal parts of the organ is almost equivalent to rendering them abortive for the present and hopeless for the future. Besides this, the dangers attending the sudden occurrence of a fit of restlessness or excitement on the part of a patient with a large section just made in his cornea, or with hooks or forceps inside his anterior chamber, are too obvious to require further comment.

Timidity or want of self-control on the part of the patient, on the one hand, and the desire that he should retain his consciousness and be ready to act according to the operator's directions, on the other, are the principal indications given by Mr. Walton for and against the use of chloroform, when its administration is admissible in other respects. Accordingly, he does not recommend the anæsthetic in extracting cataract, except in certain cases which he specifies; and this, not only on the grounds above mentioned, but because he considers it to have certain prejudicial effects with regard to the progress of the operation and the process of recovery after it; and we think he is right. He strongly recommends chloroform in operations for artificial pupil; and he pronounces it to be indispensable in the case of removal of foreign bodies from the chambers of the eye. He seems to place considerable reliance on the application of plasters over the eyes after operations involving section of the cornea, as tending to prevent the evacuation of their contents during retching or vomiting induced by the chloroform. Be this as it may, we should hope that the practice, as recommended by Mr. Walton, whether chloroform is employed or not, is not so generally neglected by surgeons as he seems to imagine.

Next comes a short chapter on the "Eye-douche," and one on "Ophthalmic instruments in general," reserving the consideration of such as are specially adapted for particular operations for the special chapters in which these operations are described. In speaking of instruments Mr. Walton says—

"Sharpness of point and keenness of edge are of paramount importance" (p. 18).

And again—

"I would suggest that scissors are not sharpened sufficiently often. I frequently see them employed when they are long past service" (p. 25).

Unnecessary as these remarks may seem, they are not the least important in a sensible chapter; for sharp instruments, if much to be desired in all operations, are absolutely essential to the creditable performance of those on the eye. We are persuaded that great embarrassment, and even downright failure, may in many cases be traced entirely to the employment of knives and scissors which, though not very sharp, are far from being so blunt as to cause any serious inconvenience in applying them to parts less delicate than the eye and its tutamina. Our author seems partial to small knives with short handles, and he gives us his reasons for it. An operator's choice in this matter must depend a good deal on individual idiosyncrasies.

Chapters IV. and V. treat of injuries from mechanical and chemical agents, and of foreign bodies in the eye. These chapters are among the best in the whole book, and will amply repay a careful perusal. We are glad to see that Mr. Walton is not infected by the modern fashion of unsparingly condemning venesection in every case—the natural reaction from the sanguinary maxims and practices of a former generation, but one involving in its applications consequences little less disastrous than those which have brought the other extreme into universal disrepute.

Here is what he says of bleeding in traumatic cases:—

"General blood-letting is frequently necessary, and is particularly applicable at the commencement of acute inflammation. It is especially indicated when there is rapidly declining vision, together with congestion or effusion in the posterior part of the eye, which is rendered probable by the bulging of the lens and iris towards the cornea, even though there should be little external evidence of inflammation. It must, however, be borne in mind that extravasation of blood will throw the lens forward" (p. 41).

His views regarding the employment of mercury are sober and judicious:—

"In my own practice, I am in the habit of using mercury in very small quantities, and almost always prescribe the *hydrargyrum cum creta*, in doses

of two, two and a half, or three grains, combining it with opium, hyoscyamus, or conium, as the bowels may require, and repeating it according to the urgency of the case; but never more frequently than four times in the twenty-four hours, and rarely so often" (p. 43).

And again—

"Modern science teaches, too, that much is to be gained by well-selected tonics; that quinine and mercury, and the use of preparations of iron, while we bleed locally, are not incompatible" (p. 45).

We are inclined to think that, among the poorer denizens of our large towns—the class most liable to injuries of the eyes—the tonic plan of treatment will be found beneficial oftener than the antiphlogistic, though this circumstance ought by no means to exclude the latter when the appropriate cases present themselves, as they frequently do.

Both in Chapter IV., and subsequently in Chapter VIII., Mr. Walton dwells on the effects produced by the entrance of lime into the eyes, and warns his readers against pronouncing too favourable a prognosis in these insidious cases:—

"A person quickly appreciates the spoiling of the cornea by the loss of vision, but he cannot have any idea of the distressing results that are to ensue when the conjunctiva alone has been severely acted on; he should, therefore, be forewarned" (p. 77).

Mr. Walton here alludes to the obstinate tendency to contraction, which manifests itself in all such cases; but he might have gone a little further, and warned us of the deceptive appearances often presented by the cornea itself, for a few days after the accident—appearances which indicate, indeed, that it has not escaped scathless, but which are far from affording to the inexperienced a fair criterion of the degree to which its future transparency, and even its vitality, may be compromised.

Our author seems to be sceptical with regard to the origin of amaurosis from injuries to branches of the fifth nerve; and he takes considerable pains to prove that, in many cases in which this might be suspected, ophthalmoscopic examinations reveal internal inflammation, attributed by him to direct injury of the eye, without external manifestation of it. We can readily believe that this may often happen, but we think it will be difficult to account for every such case in this way; and there seems to be nothing in the present state of our knowledge concerning the communication of nervous irritation which should lead us to deny that the injuries of peripheral branches of the fifth nerve may actually cause the inflammatory affections which he describes, but rather the reverse.

Chapter VI. is on sympathetic inflammation. Mr. Walton

prefers abscission of part of the eye primarily affected to its extirpation, but admits that—

“When the entire eyeball is disorganized, posteriorly as well as anteriorly, especially when there is general enlargement, extirpation is the course to be adopted” (p. 116).

The next chapter treats of caries of the orbit, and in the eighth we have the extensive subject of affections of the eyelids. Mr. Walton's operation for epicanthus appears to be much the same as that recommended by Von Ammon. He advocates the removal of a portion of the palpebral skin in trichiasis, and considers excision of the cilia and amputation of the tarsal margin, as only justifiable in extreme cases. Even in the latter case he prefers an operation similar to Jaeger's to cutting through the entire thickness of the lid, as by this means there is less sacrifice of the cartilage.

Mr. Walton attributes entropium mainly to overaction of the fibres of the tensor tarsi muscle which pass along near the edge of the lid, and he cures the affection by the excision of these fibres. He rejects the idea that the disarrangement can depend on relaxation of skin, thickening of palpebral conjunctiva, or alteration in the form of the tarsal cartilages. We are not prepared to attack either his theory or his practice; nevertheless we are of opinion that entropium may depend on different parts of the orbicularis in different cases; that abnormal states, such as those above referred to, together with certain peculiarities in the position of the eyeball, may not only supply the condition for the occurrence of the entropium, whether caused by spasmodic or normal muscular action, but also determine the part of the muscle which more immediately produces it, and even the *kind* of displacement produced by the same fibres; and that such an amount of inversion as that represented at fig. 40, p. 166, could hardly be caused by contraction of the marginal fibres, if they retained their position with respect to the cartilage, because this would involve an increase instead of a diminution of their length.

In Chapters IX. and X. we have remarks on vascular tumours. Mr. Walton recommends ligature in cases of *nævus*, where the proceeding is practicable. In regard to those cases of real or simulated orbital aneurism which are exciting so much attention at present, he says:—

“It will be an interesting subject for future investigation to ascertain the signs by which intra-cranial and orbital aneurisms may be distinguished from each other, and from obstruction to the return of blood by the ophthalmic vein. It seems probable that, in the cases which most closely simulate aneurism by anastomosis—as when the pulsation seems to spread beyond the margin of the

orbit, or where it reappears after ligature of the carotid—the cause will be found to be venous obstructions, either from pressure on the ophthalmic vein, or from disease in the cavernous sinus; and when the affection has a traumatic origin from blows on the head, it is more likely to be the carotid artery that is injured when in close relation with the bone in the cavernous sinus, than the ophthalmic, lodged in the soft tissues of the orbit; so that I should expect the intracranial origin of orbital pulsation to be by far the most frequent" (p. 238).

This, of course, does not preclude the possibility of the disease being seated in the veins, even in traumatic cases.

Chapter XI. is on tumours of the eyeball and neighbouring parts; Chapter XII. on protrusion of the eye; and Chapter XIII. on staphyloma. In operating for staphyloma of the cornea and iris, Mr. Walton advises that the lens be removed, whether it is opaque or not—a suggestion worth repeating, if not very new, for it has a considerable effect on the success of the operation. He attaches great importance to the retention of the vitreous, and mentions the practice of bringing the lips of the wound together by sutures.

Chapter XIV. is devoted to affections of the excretory lacrymal apparatus. Our author approves of Mr. Bowman's operation of slitting up the canaliculus, and he applies it to some cases of displacement of the punctum, as well as to the case of obstruction. Obstruction of the lachrymal duct he regards as generally of scrofulous origin:—

"But the disease is not an ordinary termination of strumous or other kinds of inflammation of the conjunctiva; indeed, it is remarkably seldom seen as such. I strongly suspect extension of inflammation from the interior of the nose to be rather common" (p. 344).

This opinion accords with the views of other experienced oculists, although, as Mr. Walton remarks, patients are apt to attribute the obstruction to the secondary affections of the conjunctiva.

In cases of acute dacryocystitis going on to suppuration of the sac, Mr. Walton recommends the old plan of external incision.

In Chapter XV. we have an account of the somewhat rare affection, conical cornea. Mr. Walton approves of Mr. Bowman's application of "iridodesis" to such cases. The iris is prolapsed and tied at the upper and lower margins of the cornea respectively, so as to convert the pupil into a vertical slit, the dimensions of which are reduced to something like those of a pinhole when the eyelids are nearly closed, and the dispersion caused by the faulty form of the cornea is thus in great part obviated. He has himself practised this operation with success.

Chapter XVI. treats of strabismus, or squint. We must confess that we have been rather disappointed with this chapter,

which hardly comes up to the requirements of the present day. We are aware that, in accordance with the task which Mr. Walton proposes to himself, he finds it desirable to adopt a classification of varieties "with reference to the treatment by operation," and we accept his explanation that "it is but an artificial division" which he makes use of, "and like all arbitrary tabling of disease, is necessarily imperfect" (p. 377). But we find Mr. Holthouse's classification, which is that adopted by our author, to be rather too arbitrary for our taste, inasmuch as it is founded rather more on accidental differences in degree than on differences of pathological origin. Accordingly, we feel the want of an adequate guide to the differential diagnosis of the various forms of strabismus from those paralytic and other affections which may simulate it, even when not complicated with it; and this is a matter of great importance with reference to operative treatment. However, we will admit that if our desideratum does not lie on the surface of Mr. Walton's discourse, it is involved in it, and may be extracted from it, in great measure at least, by the exercise of a little reflection on the part of the reader; and, considering the great difficulty and obscurity which still surround the subject, spite of much that has recently been done on the continent to clear it up, we are not surprised that he has given rather a secondary place to the pathology of the affection.

It is not improbable that our knowledge of this subject has been somewhat retarded, like that of general pathology in former times, by an over-anxiety to simplify, or to reduce the number of causes to the smallest possible, and even to trace up different effects to some one grand nosological principle. This is, no doubt, a temptation to which noble minds are peculiarly prone, but it is one against which we should all be on our guard; for, to speak of nothing else, the rival theories to which it gives rise present discrepancies which might easily be reconciled if we were less disposed to take one-sided views. Thus, one man attributes squinting to spasm, another to structural change, a third to defective visual power, while mal-adjustment of the focus bulks largely in the mind of a fourth. An illustration of this is afforded by Mr. Walton, who, in considering the state of vision in squint, proposes to do so under two heads:—"Impaired vision the cause of the squint, and impaired vision the consequence of it" (p. 406). In the latter category he places the presbyopic affection which often accompanies strabismus, and we see no reason to deny that it may often be a consequence; but it is no less certainly a frequent cause, and, since Donders has drawn attention to the subject, it has come to be very generally recognized as such by those who have compared his statements with cases under their own observation.

Mr. Walton operates for squint by making a small vertical incision through the conjunctiva and through the subconjunctival tissue opposite the lower border of the muscle; he then introduces a hook, secures the muscle, disengages it from the conjunctiva, and divides it between the hook and its attachment with blunt-pointed scissors. After the operation, sutures are applied to the conjunctiva. With regard to the question of operating on one or on both eyes, Mr. Walton lays down no absolute rule, but is guided by the effect of the first operation.

He attributes the horrible eversion which sometimes follow operations for convergent strabismus to too free a dissection. Cutting the muscle too far back from its attachment seems to us a more likely cause of disaster; though both faults may have their share, and are, no doubt, often combined.

Chapter XVII., on malignant affections of the eye, is a long and most valuable one. The remarks on the propriety or impropriety of operating are distinguished by much discrimination and candour.

Next follow five chapters on rather miscellaneous subjects—viz., Extirpation of the Eyeball, Artificial Eye; Entozoa and Intraocular Cysts; Operations on the Cornea; and Incision of the Conjunctiva in purulent ophthalmia.

Chapter XXIII. is occupied with the important subject of cataract. Mr. Walton first gives a description of the different forms of cataract which is clear and very practical. We must, however, enter a protest against the doctrine, propounded at p. 515, that traumatic cataract "is always capsulo-lenticular." No doubt the capsule very generally becomes more or less opaque; but not always. In recounting the subjective symptoms of incipient cataract, he says:—

"Distant things are not so well seen as near ones. This has been very erroneously mistaken for ordinary short-sightedness, and as such considered a precursor of cataract" (p. 518).

But how are we to reconcile this with the cases, which undoubtedly do occur, of persons who have laboured under presbyopia, and find that they are enabled to see well without their glasses for a time before the formation of an impending cataract? No "impairment of the adjustive power" can explain this.

Mr. Walton recommends extraction in cases of hard cataract, and condemns displacement. Soft cataracts he divides through the cornea, being, as we think, rather too strongly impressed with a sense of the risks of the posterior operation. He pronounces the following opinion on the fashionable operation of linear extraction:—

"That it is possible for the eye to be more quickly restored to usefulness by this method than by absorption, no one can doubt; but it is more dangerous, almost beyond comparison" (p. 577).

He generally removes capsular cataracts with the canular forceps.

On the whole, we think this chapter very instructive, and not the less so because the author does not pretend to lay down infallible rules for the guidance of the student in such matters as the choice of an operation, or the fitness of a case for operation—things which are only to be learned by experience, and in which even the most experienced are very liable to err.

Chapter XXIV. contains remarks on cataract glasses.

Chapter XXV. treats of artificial pupil; and it fully sustains the character of the work. The different morbid conditions which may demand operation are classified with a view to the selection of appropriate modes of operating, in a manner somewhat similar to that followed by Mr. Wharton Jones in his excellent manual. Mr. Walton's classification, though not perhaps quite so systematic as that of Mr. Jones, is distinct and practical.

Chapter XXVI. is on iridectomy and other operations for the cure of glaucoma. It is plain that Mr. Walton is not very well satisfied either with the theory or with the results of iridectomy, in so far as he has had opportunities of judging of them.

With regard to Graefe's theory, he says:—

"Indeed, if the aqueous fluid produce an injurious or disorganizing pressure, so long as the chambers of the eye are filled by it—no matter if the secretion proceed from a mere speck of secreting surface—the deleterious effect must be exerted. It also appears to me, that for Graefe's theory to be correct, the fluid should be reduced to a definite amount, actually less than the space which exists for its reception. But this difficulty occurs—What would stop the vitreous humour from becoming more hypertrophied?" (p. 640).

We must say that we cannot see the force of this reasoning. It seems to us quite possible for an eye to be full, without being too full; and we suppose that Graefe proposes to bring this about by removing to an appropriate extent what he considers the source of the over-abundant secretion. Mr. Walton, however, fairly admits that it is not necessary to condemn a practice merely because we are not satisfied with the theory which has been advanced in its favour:—

"But it must not be supposed that I am writing against iridectomy with a spirit of opposition—most certainly I am doing nothing of the kind; I am merely giving my views on the rationale that has been advanced" (p. 640).

We will admit that we are not very well satisfied with the rationale ourselves; but we cannot help stating our conviction

that, although iridectomy may have been pushed too far by some of its votaries, and ill applied by others, it is still, in its own place, a most valuable operation. Mr. Walton treats the subject in a fair and impartial spirit, and his remarks present a favourable contrast to the scurrilous attacks which continue to be made in certain quarters against German "ophthalmologists" in general and Graefe in particular—attacks unworthy alike of their authors and of their victims.

The concluding chapter gives us a very complete account of the ophthalmoscope, the manner of using it, and the more important affections which it has hitherto brought to light. We are rather afraid, however, that our author must have been napping when he penned the following passage:—

"With a little adjusting, the retinal vessels or the optic disc may be seen. The exact focus is now obtained. If, however, it should be desired to illuminate the eye still more for examination, or to magnify the parts at the fundus, the lens is now used" (p. 669).

The observer is here supposed to be at a distance* of eighteen or twenty inches from the patient's eye. If the latter is myopic, an inverted image of his retina may perhaps be seen without a lens; and if the observer holds his biconvex lens at an unusually great distance from the patient's eye, that image may be magnified; but such a concatenation of circumstances is very uncommon indeed, especially if atropine has been used. In nine cases out of ten, what the observer first sees, in spite of all his adjusting, is a confused glare; and he then brings a small inverted image—much smaller, at least, than the erect one which can generally be seen on a nearer approach—into view when he makes use of his hand-lens.

In speaking of staphyloma posticum, he says:—

"The choroid is present over the crescent, as the larger vessels may be seen dipping into the cavity, but they lose their outline by being out of focus" (p. 680).

We believe that some of the choroidal elements always do remain; but there is no doubt that, in advanced stages, the vessels may be completely obliterated so as to leave no trace. If they were present, there would be no difficulty in bringing them into focus.

We have thus taken a rapid survey of Mr. Walton's book. Its faults, to which our attention has been chiefly directed, are few and not of much practical importance; while its merits are great, and fairly entitle it to a lasting place in the standard literature of the subject. We have only to remark, in conclusion, that the wood-cuts and general appearance of the volume are all that can be desired.

III.—*Transactions of the Obstetrical Society of London.* Vol. II., for 1860. Pp. 368. London: Longman, Green, Longman, & Roberts. 1861.

NOTHING tends more to the advancement of a science than the publication of the discoveries, experiments, and experiences of the followers of that particular branch, for thereby knowledge is enlarged, and the very springs of advancement have the best and fairest opportunity of being opened up. We have received the second volume of the *Transactions of the Obstetrical Society of London*, being the volume for the year 1860. If the first volume was received with gratification, this one has been received with more pleasure. We like to see a volume of this kind; it partakes of a liberal spirit, and shows forth to the medical world the sayings and doings of a large and influential society, whose transactions might otherwise have been, as is too often the case, selfishly retained among themselves. Thus those at a distance can learn what is being done in the metropolis; and, though they may be discouraged as well as encouraged in many ways, they have the gratification and satisfaction of knowing that others, where the means of communication and consultation are so great, labour on as hard as they do to attain often the same great ends, and so the publication of such a volume as this is hailed with no ordinary pleasure.

Every book has its faults; this one is endowed with such, but it has many good qualities which overbalance the bad. We doubt not but that, in the words of the president, this volume "will take an honourable position among those of old and long-established scientific societies, and will contribute not a little to raise the character of English midwifery in the estimation of our foreign brethren."

The volume opens with the "President's address, and a memorial, on the subject of obstetrical education, to the general council of medical education." From what this memorial sets forth, the standard of obstetrical education is decidedly very low in London, as well as in some of the provinces. For instance, in London for M.D. no obstetric practice and no attendance on lectures on midwifery is required, and for M.B. the only requisite is "six cases of labour." Meagre in the extreme! Such cannot be said of Glasgow, where a course of six months' lectures on midwifery, and attendance on a certain number of cases of labour, is imperatively required.

We cannot undertake to notice fully all the papers read to the society. The first is an interesting case of intra-uterine fibro-plastic tumour removed by enucleation, by Dr. J. Hall Davis.

His patient did well. Next we have, related by Mr. Spencer Wells, a case of large congenital encephaloid tumour not impeding delivery. It was attached chiefly to the lower jaw and left side of the pharynx; brain and membranes normal.

Following this is a paper by Mr. R. Marley "On the action of belladonna upon the mammary glands." He states that he "has often been obliged to see and allow the whole process of mammary inflammation and suppuration to run their course, being unable to resist or check their progress. . . . He was determined to give it (belladonna) a fair trial." He relates forty-four cases of success in arresting mammary inflammation by its (?) use, while he mentions some cases in which he failed, "either from the patient herself improperly and carelessly applying it, or obstinately persisting in a full and stimulating diet." Though much has been said and written regarding the action of belladonna, we are of opinion that the absence of unstimulating or full diet, and the following up of his sixth direction—"keep the bowels well opened with salines, and let the diet be spare"—has more to do with the prevention of mammary inflammation than all the belladonna in the world. This drug is more useful as an anodyne, than in relaxing certain fibres, and "dilating the openings of the lactiferous ducts."

There are various cases of tumours related, all interesting in themselves, but not claiming special notice at present.

We have a paper on "Midwifery in the East," by Dr. J. Jackson; this is both interesting and amusing. Dr. Jackson does not forget the effect of the climate on Europeans who go out to India, and gives some useful hints on that subject, more especially referring to the female race, upon whom the relaxing nature of the climate generally operates injuriously at first. Labour generally lasts only a few hours, and "the patient is kept in the recumbent position, or in bed, until the tenth or twelfth day." It is dangerous for Europeans to get up earlier than this, though among the natives many are actively at their household duties on the third day. He mentions that a bandage formed by a long sheet or obstetric binder is "used during labour, and tightened from time to time; so the uterus contracts and the fœtus is expelled, and thus the placenta is rarely retained, but comes away in a few minutes after the child."

Hour-glass contraction, with flooding and post-partum hæmorrhage, are common accidents during parturition among Europeans long resident in India. Trismus infantium is prevalent; uterine diseases are not infrequent. Diarrhoea and dysentery are very common with pregnant women in the East, as well as vomiting. Phthisical patients become invariably worse during pregnancy.

Amongst the poorer class the mode of conducting parturition is simple enough, while "with the higher class of native women, where caste prevails, the poor creature is considered an outcast at this particular time; and, instead of having every tenderness and affection lavished upon her, as in this country, she is placed during the pangs of labour in the meanest hovel in the yard, as an unclean creature; or she is put into a matted shed in a lower verandah of the house, where, lying upon the floor, she is exposed to the heated fumes of wood and charcoal fires, which induce a degree of stupefaction and anæsthesia." The posture of the woman is on her back, with outstretched legs, while the midwife is seated in front, somewhat after the French fashion.

On the whole, this paper is replete with interesting information, and Dr. Jackson gives a slight insight into the customs of the East, in addition to the strictly professional part. Another such paper will be interesting at a future occasion.

Cases of abnormal gestation are not wanting in this volume; they are interesting as curiosities, and unfortunately such cases occur so seldom that no rules can be set down for their treatment. Mr. Henry Grace gives a case of fallopian pregnancy; Mr. R. Hardey, of pregnancy without development of the lower segment of the uterus; and Dr. Drage relates a case of extra-uterine pregnancy.

There is a very useful and able article on a subject which has been almost totally neglected by our standard writers on midwifery—viz., *concealed accidental hæmorrhage*. Dr. Hicks has done good service in elucidating cases of this kind; and though the number is small comparatively (twenty-three), yet the results are interesting to all. The difficulty of such cases renders our acquaintance with their symptoms very necessary, more especially as these symptoms are chiefly negative in character, and the ultimate issue is too often fatal. According to Dr. Hicks, of twenty-three cases, fifteen died, and eight recovered. We hope that this subject will be more fully cleared up than it has hitherto been.

Mr. Hardy has a paper "on special position and the obstetric binder as aids in the treatment of impeded parturition." The obstetric binder is nothing new, being merely the ordinary binder applied during labour, while the "special position" is that of the sitting posture, as "not inaptly illustrated by the act of defecation as performed under circumstances of difficulty and distress." Cases are given of the advantage of this position in difficult cases, and according to Mr. Hardy the use of the forceps has been often superseded; most of his cases, we doubt not, would have yielded in due time if the attendant accoucheur had "had patience." The article is worth perusal.

"Degeneration of the Ovum," "Menorrhagia," "Fœtal Peritonitis," "Propagation among the Industrial classes," follow. The latter paper is interesting, and from the pen of Dr. Granville.

Many other valuable papers follow, which we cannot take notice of at present, but commend to the perusal of our readers.

After the accounts of some "Monstrosities" the volume finishes with a paper "on anæsthetics in midwifery," from the pen of Dr. Kidd.

Viewing the subject as he has treated it, we are still not disposed to concur in his views; and though liberal in sentiment, on this point we are conservative, and till much larger statistics and clearer rules are laid down we consider "chloroform is full of danger, and that as our great-grandmothers did without it, our daughters or wives ought to do the same," and this in the face of his statement that "there have been no accidents in about 40,000 obstetric cases under chloroform," an assertion which we cannot but receive with some hesitation. As for the style of this paper, it is somewhat confusedly written, and if it is difficult for one born in this country and educated here to understand it, surely it will be so "to our foreign brethren."

On the whole the volume is well worth perusal; it has been got up with care; and the printer's part is well executed.

IV.—*Metanoia—A Plea for the Insane.* By HENRY M'CORMAC, M.D., &c., Belfast. Pp. 16. London: Longman, Orme, & Co., 1861.

THE author of this pamphlet endeavours to prove that insanity is a psychological and not a pathological disease—an abnormal condition of the soul, not one of the brain. He admits, however, that corporeal disease, nerve-deficiency, may lead to insanity, and is often associated with it, "but contends that corporeal disease is not insanity." We are not aware of any writer who maintains that structural disease constitutes insanity; doubtless many regard mental disorder as the result of lesion of the brain-substance, but this is quite a different matter from confounding the effect with the cause. No man would assert that in severe neuralgia, the altered condition of the nerve-substance *was the pain*; the latter is merely the effect of the structural derangement. In the same manner insanity is often the indication of cerebral disturbance. On the other hand we agree with Dr. M'Cormac in thinking that "it is not true that a change in the material organism

is necessarily involved" in insanity. Our author resolves sanity and insanity into what he denominates "fixity of impression," and concludes—"So, thus congruous with the laws of consciousness, it is fixity of impression, the appliances being conformable, that characterizes, indeed creates, sanity in the sane, and in the insane insanity." We hope our readers are sufficiently versed in metaphysics, to comprehend this passage. The pamphlet abounds in many such, and will prove a real treasure to the student of mental science; it will afford him ample scope for his critical acumen. Dr. M'Cormac's object, however, is a most praiseworthy one. He aims at establishing a doctrine which will secure to the insane a moral, intellectual, and physical mode of treatment; the latter being subservient to the two former, and conducted not by unlettered keepers, but by educated, well trained, and humane attendants.

The mode of expression which Dr. M'Cormac adopts is so peculiar, that we cannot help quoting one or two passages:—"The notion that insanity is merely a cerebral disease, a disorder of the vesicular neurine, or nerve-substance, is only fit for that limbo to which Milton consigns things shapeless, fantastic, and unreal" (p. 5). Again, "Of a surety, the bodily health must be seen to in insanity, phosphorus must be furnished but in copious food supplies, disordered cerebral action, when it subsists, must be set right" (p. 11). Such sentences are frequently to be met with in the work, and will certainly appear to many to be very *disorderly* combinations of terms.

We must conclude with the quotation which embraces the principles of treatment for which the author contends, even in cases depending on cerebral disease:—

"Yet even here, moral treatment is the one essential thing. If disease, indeed, be urgent, if the nerve-structure be lesed, and its function seriously impaired, the man must die. Yet, short of this, the potentiality of Recuperation, of Self-Integration, if I may coin an expression, never wholly intermits, and *moral influences* come ceaselessly into play. Substitution is the great agent for reclaiming the insane. This going out of self is the moral lever, the mighty engine, which is to raise the ruined soul, supplying such allurements as lie within our reach, till at length the principles of Self-Assertion and of a wholesome Autonomy being roused, the soul, its nobler powers awakened, gazing face to face on self, is rescued again" (p. 11).

Though we cannot commend this pamphlet on account of style, or of the cogency of arguments, yet we think that Dr. M'Cormac deserves much credit for advocating so forcibly a humane, intellectual, and moral plan of management of insanity—a system which will receive the support of every enlightened physician, whatever his views may be regarding the relation which this most deplorable malady holds to cerebral disease.

V.—*Lectures on the Diseases of the Kidney, generally known as "Bright's Disease," and Dropsy.* By S. J. GOODFELLOW, M.D., London, &c., &c. Pp. 306. London: Robert Hardwicke. 1861.

WHEN we reflect on the amount of labour which has been bestowed on the study of diseases of the kidney since Dr. Bright first wrote on the subject; when we consider the extensive discoveries which have been made during these investigations; and when we compare the present state of our knowledge on this important class of cases, with that which our predecessors enjoyed—we cannot fail to feel proud at the advanced position to which we have attained, and at same time find it very difficult to understand how such diseases could have been properly treated, at a period when their pathology was so obscure as to be nearly unknown. If we were asked, to whom do we owe the present advanced state of our knowledge? our answer must be, Dr. Bright, who was the first to direct attention to the state of the kidneys in dropsy, and whose graphic and truthful delineation of the appearance of the renal organs, and description of the symptoms, have not been surpassed by any subsequent writer.

But if we were asked, to what do we owe it? we have simply to answer, to the study of *morbid anatomy*, or in other words, to the tracing of the connection of symptoms manifested during life with the pathological or anatomical lesions found after death. It was this principle by which Dr. Bright was guided in his investigations, and by it he gained his important achievements.

This fact cannot be too prominently kept in view at the present day, when by a certain class of writers, the anatomical lesions are all but ignored, at least kept in a most subordinate position to that which these writers are pleased to denominate Pathology, or pathological science. It must not be for a moment imagined that we wish to disparage the study of pathology in its widest and most philosophic bearings; on the contrary, we are anxious to aid it onwards by our most strenuous commendations, but we regret exceedingly that the study of the *morbid conditions* is by far too much overlooked, in the higher effort to penetrate into the processes by which these anatomical lesions have been produced. Morbid anatomy, however, is only the foundation on which scientific pathology can be based. It was in consequence, we repeat, of proceeding on this sound, and we may add, self-evident principle, that Dr. Bright made his important discoveries. And whatever of real value has been added by subsequent labourers in the same department, has been secured by proceeding on the same principle—viz., by calling to their aid the microscope

and test tube, and by such important agents prosecuting their investigations into the ultimate elements of the *morbid states* pointed out by Bright.

Bright with the dissecting knife made known the altered conditions of the kidneys. Johnston, Gairdner, Virchow, and others, by the microscope have elucidated the histological alterations; and consequently we have been enabled to classify "Bright's Disease" under the distinct and separate lesions of "large white," "contracted," "fatty," and "waxy or amyloid kidney."

The labours of these able men have not been directed only to the investigation of different pathological states, but they have at the same time endeavoured to ascertain the symptoms by which during life, we can predicate which of the lesions is present in any given case. In this department they have been ably assisted by Rees, Wilks, Basham, especially the latter, whose excellent work furnishes us with the means of diagnosing the various morbid conditions with great certainty.

Whilst Dr. Goodfellow in his "Lectures" brings the whole subject before the reader, he also especially aims at enabling us to diagnose the different lesions during life, and, as we will have to point out in the sequel, this constitutes the most valuable part of his work. We fear that the reader will not meet with much that is original in these lectures. But though no new fact of any moment has been adduced by the author, yet he brings the different lesions before our notice in such a very systematic and logical manner, that the work cannot fail to prove exceedingly instructive, either to the student, or to those members of the profession who have not had an opportunity of keeping up their knowledge of the various discoveries which have been made of late years in renal pathology and therapeutics. We will now proceed to give a brief analysis of the work.

Lecture I. is devoted to the consideration of the symptoms of the disease; though there is nothing new, yet we deem his concluding summary worthy of quotation:—

"So far as general symptoms are concerned, we are now in a position to point out more consecutively, and in 'closer order,' the direction in which your general clinical examination should be carried on, and the manner in which your notes of any case recorded.

"First, then, anasarca. Now you will perceive that I have termed this condition a symptom; that it is a sign of a well-understood condition there is no doubt, and you will have observed that I have given it as a very early symptom. It may be as well to state, that it has been termed a 'secondary affection' by some authors; but, as I conceive, erroneously. I have already said that it is one of the earliest signs of Bright's Disease. In a very large proportion of the cases—in 99 out of every 100 of the cases—it is the first intimation of the disease; and although I do not mean to deny that other symptoms less definite

in the information which they convey to the mind, may have preceded it, yet they are of trifling value as compared to this, and often escape the patient's and the practitioner's notice, until this symptom or condition has made its appearance. With reference to this symptom, in taking notes of these cases, it is necessary to describe as far as possible how it came on, what parts of the body exhibit this condition, and as a general rule, it would be well to take the measure of the extremities, both after the patient has been in the upright position for some hours, and after he has been lying in the horizontal position in bed, so that we may be able to see and judge of the progress of the case.

"Secondly. The pallor and emaciation, as 'confirmation strong,' that the preceding condition, the anasarca, is due to kidney disease.

"Thirdly. As to whether there be any pain in the loins, and if so, what its nature is, how far it extends, and whether increased under pressure and motion, and what organs and parts it involves, and how it does so.

"Fourthly. As to the quantity of water passed daily (which should always be recorded), and frequency of micturition. The specific gravity and other properties I shall enter more fully upon after I have described the condition of the urine generally.

"Fifthly. As to the heat or scalding in passing urine, and whether there be any discharge from the urethra. This will enable us to separate what we may call the extraneous matters from the true kidney-constituents of this altered urine.

"Sixthly. As to the state of the skin, as to dryness and heat, and the condition of the cuticle, the presence of eruptions, and so on. And

"Lastly. As to the pulse, and with this the general symptoms indicating a feverish condition of the body—chilliness, rigors, headache, thirst, &c. And in the chronic forms, your clinical history would be very far from complete if you did not inquire into and take note of those symptoms, which I have mentioned as having reference to remote organs, some of which are generally present; and in particular those symptoms and signs indicative of the secondary affections, especially those of the heart, lungs, and brain, which are of such great interest and importance, and which play such an important part in the later phenomena of the different diseases which we are now considering" (pp. 19 and 20).

In Lecture II. we have the general condition of the urine, and the blood in the disease very fully examined. All that is known on these most important points is brought under the notice of the reader. We deem the following remarks entitled to special attention:—

"Besides the albumen and other constituents of the serum of the blood (for I look upon it that its presence in the urine is principally owing to a simple transudation of the serum into the uriniferous tubes and Malpighian capsules), we may have blood-corpuscles and blood-casts, fibrinous filmy matters and casts of tubes, epithelium particles or epithelial and other kinds of casts, granular, fatty, waxy, and so on, and occasionally (and especially in advanced stages), puriform mucus-globules from the urethra, bladder, ureters, and even pelvis. These casts and adventitious-constituents, with the exception of albumen, are only to be seen by the microscope, and to recognize them with certainty is one of the most valuable accomplishments which you can attain; and I would emphatically recommend you never to lose an opportunity of making yourselves practically acquainted with these casts—measuring their diameter, and so on. Here are diagrams of all the different forms.—(See Plate I.) No diagrams will convey an idea of their appearance under the microscope like that which you will derive by examining the tube-casts themselves, as well as the other

constituents, the different kinds of epithelium, the tessellated and columnar from the bladder, the columnar from the ureter, the smaller and fine tessellated from the pelvis, the globular and glandular from the convoluted tubes, and the same kinds of epithelium, but somewhat flatter and more resembling the scaly variety, from the straight tubes. Of course, with this you may have both scaly and columnar epithelium from the urethra and from the vagina. It is important also to be able to recognize the extraneous matters that are always found in the urine, such as cotton fibres, blanket hair, &c.—(See Plate I.) But a perfect acquaintance with the casts is essentially necessary; for it is by the character of these casts, when taken in conjunction with the history and symptoms, that you will be able to discriminate the several affections which we are now considering, and which I shall describe more at length in a future Lecture" (pp. 47 and 48).

We beg to express our hearty concurrence in this recommendation. The microscope is as essential in the detection and proper management of renal disease, as the stethoscope is in the diagnosis and treatment of thoracic affections. We are exceedingly glad to be able to state that in the wards of the Glasgow Royal Infirmary, this instrument is in daily use in the investigation, not only of renal diseases, but in every other affection in which its application is practicable. The importance of this kind of instruction is immense to the students who attend this institution.

The alterations in the blood he sums up as follows :—

"In order to put these alterations of the blood in the acute stages in a form convenient for reference, they may be generally characterized :—

"1st. By an increase in the fibrin.

"2nd. By a diminution in the quantity of albumen.

"3rd. By the presence of urea.

In the chronic forms :—

"1st. By a normal, and, under some circumstances, increased quantity of fibrin.

"2nd. By a diminution of the albumen in the serum, varying in amount according to the state of urinary excretion.

"3rd. By a progressively advancing diminution of red corpuscles.

"4th. By a slight increase in the soluble salts.

"5th. By more or less accumulation of the excrementitious constituents of the urine" (pp. 53 and 54).

In Lectures III. and IV., he discusses the proximate causes of the symptoms. Though the reader will find in these lectures much ingenious reasoning in the attempt to elucidate these very obscure points, yet we fear that the conclusions will not appear very satisfactorily established. He ascribes the symptoms chiefly to the presence of a large quantity of urea in the blood. For example, anasarca is thus accounted for :—

"Having come to the conclusion that either urea itself (as I am led to believe) is poisonous, or that the urea and extractives together are so, in what way do they act in the production of anasarca? I cannot agree with Fretichs in the opinion that acute dropsy—that after scarlatina, for example—has the same cause as the kidney disease, and that it is in consequence of the paralysis of the capillaries

of the skin and subcutaneous tissues excited alone by exposure to cold. I do not believe that there ever was a case of general dropsy from mere exposure, unless the kidney had first suffered from the exposure, and its function been more or less suppressed. I have already indicated the order in which I think the dropsy most commonly, if not invariably occurs, whether after exposure to cold or from any of the other causes of kidney affection leading to dropsy. In the acute forms it is one single poisonous substance which produces what has not been inaptly termed inflammatory dropsy, as well as the more severe nervous symptoms which we witness. There are two ways of explaining the *modus operandi* of these agents, or rather of this agent, in causing dropsy. The circulation is undoubtedly retarded, perhaps arrested altogether, in some parts of the capillary system. Most probably it is in the venous capillaries that this retardation takes place—that which is furthest removed from the influence of the cardiac impulsive pressure, and the constant arterial pressure. One way in which the uræmic poison may act is by directly affecting the capillary vessels themselves, relaxing their walls, and converting them in a manner into inert tubes. . . . The other way of explaining this effect upon the capillary blood-vessels is by supposing that the poisonous agent in operation first affects the nervous system, and by destroying or impairing its influence upon these vessels, relaxes their walls, and interferes with the circulation mechanically in this way; and in addition, retards it by preventing the action of the chemical affinities which we all know to be so influential in carrying on the circulation through the capillary system" (pp. 71 and 72).

The pallor, and other symptoms, he ascribes to this toxic power of urea, and the consequent disordered relation of the elements of the blood.

We are inclined to think that our author places too much weight on the noxious properties of urea—a substance which, as he admits, is always present in the blood. He affirms, however, that the mischief arises from the greatly increased quantity present in the disease. With regard to those cases which have been reported by Dr. Rees and others, where a very great increase of urea was detected without any symptoms of Bright's disease, Dr. Goodfellow explains away their bearing on the question, by supposing that the accumulation of the urea took place slowly, and that consequently the constitution had become gradually accustomed to its influence. We do not think that we are as yet in a position to pronounce any decided opinion on this alleged poisonous influence of urea—the subject requires further careful investigation.

Lectures V., VI., VII., and VIII. are devoted to the consideration of the causes of the disease, viz., scarlatina, cold, and alcohol. He enters minutely into the physiological and pathological effects of these agencies, and endeavours to show how they produce disease of the kidney. In Lecture V. he controverts at some length the generally received opinions regarding the physiology of the secretion of urine. He observes:—

"You will find that it is somewhat opposed to the theory which Mr. Bowman promulgated, from a careful study of the anatomy of the kidney, as revealed by

his extensive, patient, and admirably-conducted investigations, and which was almost universally accepted as true by physiologists, and has ever since been taught in the schools. Many of those, however, who even teach this theory in their lectures, are not exactly satisfied with it, and yet are not prepared to state definitely in what they dissent from it. I must confess that I have not been able to give my faith to it for some time past. There are so many difficulties in explaining many conditions under this theory, that I have been led to conclude that, ingenious as it is, and true undoubtedly in many respects, yet that it does not comprise the whole truth. According to this theory, then, it would appear that the retardation of the circulation, and consequent increase of pressure, which the peculiar arrangement of the Malpighian vessels is so eminently calculated to produce, lead to the separation 'of the fluid portion of the urine' by a process of filtration, while the solid matter, composed of various organic constituents and inorganic salts, is separated by the aid of the glandular epithelium, which lines the convoluted portion of the tubes. The water is supposed to come from the blood in the Malpighian vessels by a process of filtration, and the organic constituents and the inorganic salts from the blood in the capillary network surrounding the convoluted tubes, by a true secretion, through the epithelium lining the tube. Now, in examining closely this theory, we must conclude that the blood in passing through the Malpighian capillaries, where the circulation is most retarded, and where the greatest lateral pressure must necessarily be exerted upon their walls, only parts with the water; while the network of capillaries which receives the blood from the small efferent vessel, where the lateral pressure must be very much less, permits the transudation through their walls of ordinary blood plasma, together with the 'organic constituents' and the 'inorganic salts' of the 'secretion,' in order that the 'true secreting elements' should separate the latter. In order to accept this as true, the capillaries into which the efferent vessel breaks up (the Malpighian capillaries) must have much thicker walls than the capillaries which surround the tubes, and which are formed by the efferent vessel. This difference, if there be any, I have certainly never been able to discover" (pp. 147 and 148).

We feel very much inclined to coincide with Dr. Goodfellow in these and the other objections which he adduces, and also to agree with him in his physiological views on the subject; indeed, many years ago we have expressed elsewhere opinions almost identical with the following:—

"My own idea is (and I mention it with great diffidence, for my subject is not physiology), that, under the combined influence of pressure, quality of blood, and the nervous force, the urinary constituents are separated directly from the Malpighian capillaries, and that whatever constituents of the serum or of the blood are normally transuded through their walls, are *absorbed* by the epithelial cells of the tubules or by some other agents before the convoluted tubes become continuous with the straight or simply excreting ducts; and that the blood, purged and deputed, which leaves by the efferent vessel, while passing through the network of capillaries in the tissues of the kidney, parts with the normal plasma for the usual nutrient processes, as in other organs.

"It is the common practice to speak of these tubules or channels as mere excretory conduits. This leads to very erroneous notions of their true office. It is probable that the separation of the urinary constituents from those of the serum that may be transuded with them actually takes place in these tubules, and that it occupies some length of time before the urinary constituents, transuded from the Malpighian capillaries, find their way into the straight ducts. These channels are of extreme length, and highly convoluted. I am disposed

to regard them more as like the blind tubules of the stomach and other mucous membranes, than as excretory passages—with this difference, that instead of terminating in blind extremities, they communicate with the straight, purely excretory tubes by an open orifice. Not only is a separation of the constituents of the urine probably effected in them, but those matters which are required for the system, and which cannot be lost without detriment, are reabsorbed, just in the same manner that the constituents of the gastric juice, after they have performed their office, are reabsorbed in the lower portions of the intestinal canal for future use; for none of these constituents are normally found in the feces. We have an analogy for this manner of secretion in some, at least, of the secretions—*e. g.*, the gastric and biliary" (pp. 152 and 153).

In Lecture IX. he describes the different forms of diseased kidney, usually included under the general term, Bright's disease. He adopts the view that there are two essentially distinct typical forms of the affection, *viz.*, the large white kidney, and the small hard contracted kidney; but he states:—

"In entertaining this opinion, however, I do not ignore the fact that the process which leads to the one form may be engrafted upon the other, or that from special causes, constitutions and states of body, the two processes may go on contemporaneously, and so form a mixed kidney—that is to say, a kidney having a more or less close resemblance to both these forms, either in the same or in different parts" (p. 196).

After giving a very fair description of the different appearances which these two forms of disease present at their different stages, he next proceeds to point out the symptoms by which they may be diagnosed during life.

He seems to entertain the opinion that the large white kidney is a sequela of scarlatina, consequently he considers acute anasarca after scarlatina as pathognomonic of the acute stage of the large white kidney. This opinion is quite opposed to our experience. We have found the small contracted kidney very frequently after scarlet fever, and, on the other hand, have often met with the large white kidney independent of any such affection.

With regard to the symptoms which he considers indicative of the chronic stages of the large kidney, we beg to express our doubts of the value of the general phenomena, which he mentions because the greater number are met with in all forms of the disease; but we have every confidence to place in the microscopical character of the urine, especially the "fibrinous cylinders" or "casts;" some being "clear and small," others "granular," and others again "studded with imperfect and granular renal epithelium;" and frequently both the cells and all the casts will be found to contain more or less oil particles in varying sizes, some being extremely minute.

"If there happen to be many tubes of large size, and if there be a considerable amount of oily matter in them, it will be a certain sign that the kidney is

in a far advanced stage of disease. The albumen is generally abundant, but less so than in the more acute stages of this form of kidney, and forms a much lighter and finer precipitate on boiling, and by the addition of nitric acid" (p. 205).

The Small Contracted Kidney.—With regard to symptoms indicative of this affection, he omits to notice those belonging to the acute stage, viz., the febrile excitement, the smoky urine—loaded with blood corpuscles, the dark tube casts covered with tubular epithelium, and lastly, the general anasarca.

In the chronic form, he tells us that "the symptoms are obscure and of a secondary character." This is true only to a certain extent. Indeed, as a rule, the general symptoms differ very little from those of the other form; the dissimilarities on which most reliance should be placed will be found in the urine when placed under the microscope, the tube casts are fewer, smaller, often perfectly hyaline, and with little or no oil globules. These circumstances will be found to be noted in the cases published in the work of Dr. Basham. The following statements also are open to many exceptions:—

"There is very seldom any anasarca. If there be any, it is small in amount, and then generally only at the close of life. But in many cases, on looking attentively, you may, as I have stated in a preceding lecture, detect a slight oedema beneath the conjunctivæ and in the feet towards evening, and a general puffy condition of the eyelids and of the loose subcutaneous tissues. The more positive or evident symptoms and signs of this form are pallor, neuralgia, headaches resembling those characteristic of hemicrania, noises in the ears, moles before the eyes, and other symptoms referrible to the nervous system, dyspeptic and other symptoms indicative of gastric and intestinal irritation, and even inflammation; for even extensive ulceration of the stomach is occasionally found with this condition of kidney, but whether it be an effect of it, experience has not enabled me to decide, although it is not improbable" (p. 208).

Though, doubtless, many of these symptoms are often present, and claim the patient's chief attention, yet they are far from being the "more positive or evident" signs. When, however, a patient applies for advice, labouring under such a train of symptoms, the condition of the urine should be carefully examined; and if renal disease be present, the test tube and microscope will at once demonstrate its existence. Dr. Goodfellow states that the urine is as abundant, or even more so, than in health, in such cases. On the contrary, we have found the quantity invariably reduced, often amounting only to one or two pounds daily.

Dr. Goodfellow describes a variety under the term "mixed form"—that is, a kidney partly of the large white, and partly of the small contracted character—in other words a combination of both; but Frerichs would denominate it a mere transitory stage of the same degeneration. We are strongly inclined to agree with the latter authority, as being the view most consonant with

degenerations in other organs and tissues, resulting from changes taking place in inflammatory exudations. The symptoms which Dr. Goodfellow quotes as diagnostic of this so-called mixed form, differ little from those peculiar to the large white kidney—indeed the microscopic appearances are identical.

He next briefly alludes to the “waxy, lardaceous, or amyloid kidney”—a form of disease which has recently been much investigated. Dr. Goodfellow states that “this variety must be rare, for he has not met with it in his practice in the hospital, in such a way as to be evident to the naked eye.” From this we infer that he has not availed himself of the only means by which the degeneration could be detected, viz., solution of iodine and sulphuric acid. “No doubt,” he adds in a note that he has been informed by Mr. Sibley, “that this form of kidney is often found in the dead-room in scrofulous subjects,” &c. For our own part we have found the waxy kidney of frequent occurrence, especially in cases of phthisis, and in other exhausting diseases.

In our experience we have often found nothing abnormal in the urine—no alteration of quantity or specific gravity; no albumen, though carefully searched for; no tube casts. In some cases, however, the quantity of urine was increased at first, and albumen existed in considerable quantities.

With regard to the seat and nature of this degeneration, it seems quite established that the minute arteries are the seat; but as to the nature of the alteration, as yet we have no definite information. The new substance seems to be nearly related to cellulose—but we beg to refer to our April number for further information on this important pathological degeneration.

We deem it useful to quote for the benefit of our readers, the method proposed by Dr. Harris for the detection of this amyloid change (*vide Lancet*, vol. ii. 1859, p. 639):—

“A thin section of the kidney should be made either with Beale’s or Valentin’s knife, carrying the section through the cortex well into the pyramids; this section is then to be spread upon a glass slide, and washed with a stream of water. After having been examined with one-inch power, a small drop of a solution of iodine, composed of twelve grains of iodine, twenty-four grains of iodide of potassium, in one fluid ounce of distilled water, should be placed on the side of the slide, and then diluted with two large drops of ordinary water. Then allow the solution to run over the preparation, and immediately the Malpighian bodies will be seen to assume a bright carmine colour, when viewed by transmitted light, if this degeneration be present. This colour, which may have a slight orange tint, if the specimen be viewed by daylight, will be developed gradually, and will not, as a general rule, attain its maximum in less than a quarter of an hour. The solution of iodine is now to be allowed to drain off, the preparation is to be covered with thin glass, and the space between the thin glass and the slide to be filled up with ordinary water. A single small

drop of the dilute sulphuric acid of the Pharmacopœia is then to be placed on the edge of the thin glass, and as it slowly diffuses itself through the water the colour instantly begins to change, becoming at first more of a port-wine colour, and then gradually passing through the various shades of the colour recently become so fashionable under the name of "red mauve," till it finally assumes an indigo-black. The parts, however, on which the acid acts less completely, still retain a tinge of violet."

The description of the fatty kidney is brief, but correct. He observes :—

"In most, indeed in all, of the other forms of these affections, the kidney may undergo the fatty metamorphosis, but especially is it prone to take place in the large white kidney, and in the mixed white and granular kidney. Virchow has found that the kidney, whose epithelium has passed into a fatty degeneration, nearly always shrivels up, and the result is a permanent atrophy. But when the pure typical white kidney undergoes this metamorphosis there is but little diminution in size, and the exudative matter deposited in the tubules and intertubular substance seems to undergo a still further degeneration into fatty and oily matter. This condition will generally be found in persons who have been addicted to intemperance, especially in the use of undiluted spirits—as brandy, gin, and whisky.

"The symptoms indicative of the fatty kidney will be more or less modified according to the conditions of the organ with which this metamorphosis is associated, and also with the greater or smaller amount of this degeneration in other organs of the body, especially in the heart and arteries. There is generally very considerable anasarca, although great fatty degeneration of the organ has been found where no anasarca was observed during life. The same may be said, also, with regard to albumen. When the white kidney is the seat of the metamorphosis, you will invariably find considerable anasarca, and more or less of albumen, and of fat or oily casts in the urine" (p. 212).

Lecture X. is devoted to a consideration of the pathological processes which produce the different forms of renal degeneration. On these points there is no new fact adduced; the usual explanations are given, but in addition our author employs much ingenious reasoning in the application of the different items of the knowledge, which we do possess.

In Lectures XI. and XII. we have the treatment of the different forms fully discussed.

In the general prefatory remarks he makes many important observations; for example :—

"You will there learn how long a time is necessary, in the chronic forms more especially, before any appreciable results from the treatment can be obtained, and yet how necessary it is to persist in it from week to week, and even from month to month. Young practitioners expect an immediate result from their remedies, and are disappointed if they do not meet with it, and are therefore induced to change their mode of treatment far too often. The consequence is, that every remedy and every plan of treatment fails. In chronic diseases it requires (what one may call) an obstinate persistence in the remedies and treatment, even although we do not immediately observe any very apparent effects." (p. 247).

Such facts cannot be too strongly urged on the attention of young practitioners.

He divides the treatment into the following heads :—

“First, the acute forms ; that due to scarlatina, to cold, to alcohol (an excessive and long-continued debauch), and that due to the action of other irritants, whether general or local.

“Secondly, the chronic forms, according to the same arrangement” (p. 254).

As preventives of the scarlatinal form he recommends proper ventilation, cleanliness of the apartment, whitewashing the walls, &c., and quotes some remarkable instances of the value of such measures ; he also observes :—

“There is one other point connected with the treatment which I feel bound to refer to, because I think that it is influential as a preventive of these diseases, as well as eminently curative of the scarlet fever itself. I allude to the practice of giving tonics, such as quinine and iron, or iron alone, in tolerably large doses, proportioned to the severity or malignancy of the disease. I have been in the habit, for many years past, of prescribing these tonics in bad forms of scarlatina, securing, at the same time, a moderate but still efficient relief to the bowels once or twice daily by castor-oil, or some other mild aperient, with the most favourable results ; nor (which is of more interest in connection with our subject) has any patient become affected with dropsy, so far as I have been able to ascertain—certainly not in consulting practice. In hospital practice one cannot be so certain, for it is possible that some may have been attacked after they have been dismissed ; but as most of the patients come from the neighbourhood of the hospital, it is most probable that they would have returned if they had been attacked with dropsy” (p. 261).

With regard to the treatment of scarlatinal dropsy, he places most reliance on bleeding ; especially “when the patient is threatened with convulsions soon after the commencement of the attack, this should never be omitted.” We can corroborate this from extensive experience on the point. We also coincide with the remark that, if the patient be exhausted, depletion must be effected by leeches or cupping. He places the warm bath next in efficacy, but our experience of this remedy has not been favourable ; we prefer repeated doses of the genuine James’ Powder, or solution of antimony. We cordially join with him in advocating an efficient aperient early. Croton oil we have found by far the most efficacious :— *

“These, then, are the great and immediate remedies that will be called for in most cases. The extent to which they should be carried, or whether one alone or more need be resorted to, will depend on the urgency of the case. This must be left to your own judgment. Bear in mind, as a golden rule in the

* It might be interesting to the author of a late article in the *Saturday Review*, to know that this use of croton oil was made known to the writer by a country practitioner—Mr. Herbert of Airdrie.

treatment of all diseases, that it is better to err on the side of too little activity in the treatment than on that of too much" (p. 265).

We would add that counter-irritation to the renal region is a most important remedy in the acute forms of the disease. Repeated sinapisms, or the use of nitrate of silver, so as to destroy the cuticle, have invariably, in our hands, produced most valuable effects. After the acute stage has passed, and after the tube casts have disappeared, and when the febrile excitement has subsided, we agree with the author in recommending the tincture of the sesquichloride of iron.

When the disease results from cold, he recommends the same line of treatment, and remarks that patients bear depletion better; in which opinion we fully agree. Bleeding, either general or local, proportioned to the strength of the patient, we have found most valuable; it should be followed up by diaphoretics, aperients, and counter-irritation to the renal region.

In the treatment of convulsions, he recommends general or local bleeding, according to the duration of the attack and the strength of the patient; he also advises a blister to the scalp, with a brisk hydrogogue cathartic. We concur generally in this advice, and can especially recommend the use of croton oil in such cases. In these convulsive attacks we have often witnessed the happiest effects, from a general bleeding (in adults), followed by cupping the loins, and a dose of croton oil.

In the sub-acute forms we agree with the author in regarding the treatment as more complex. The action of the skin must be promoted, the bowels must be freely opened, and—so long as the dark tube casts are met with in the urine combined with the smoky hue and blood corpuscles—cupping or leeching the loins to an extent commensurate with the strength of the patient. The tincture of the sesquichloride of iron is strongly recommended by Dr. Goodfellow after the urine has assumed a more healthy aspect. We can also speak favourably of its effects. He recommends highly a combination prescribed by Dr. Basham, namely, *Liquor ammon. acetat.*, from one to two drachms; *acidi acetici diluti*, twenty minims; *tinctura ferri sesquichloridi*, ten to fifteen minims, given as a draught in one ounce of water. We usually either prefer ten to fifteen drops of the simple tincture of the sesquichloride in water, or the following mixture:—*Liquoris potassæ*, two drachms; *potass-tart. ferri*, one drachm; *infus. cascarill.*, twenty ounces; *syrup of ginger*, two ounces; and of this mixture a wine-glassful is given twice or thrice daily.

In the treatment of the chronic forms, Dr. Goodfellow remarks:

"In these cases, notwithstanding that the structure of the kidney is permanently damaged, we may, nevertheless, by appropriate treatment, medicinal

and dietetic, restore to a great extent the function of the organ, reduce the anasarca, and very much improve the general condition of the patient" (p. 274).

On this important point we beg to express our entire concurrence; such patients are often left to time and nature, to sink into what may in reality be termed a premature grave.

He recommends dry cupping the loins, small leechings, counter-irritation; but when there is much anasarca, for obvious reasons, neither dry cupping nor blistering should be employed. Iron and tonics, purgatives, and the other customary remedies, are recommended by our author to be used prudently, and in reference to the peculiarities of each case. The following advice is worthy of attention:—

"Mercurial preparations are contra-indicated in these diseases, and are highly injurious. Occasionally, a grain or two of blue-pill, or the hydr. c. cretâ, may be usefully combined with other aperients in the form of a pill; but in general, other cholagogues, such as taraxacum, the mixed acids, with a vegetable bitter twice a day, will be better, with the mild aperient pill given at night" (p. 283).

We have witnessed the most serious consequences from the use of mercurials in such cases; their employment cannot be too strongly condemned.

In the fatty kidney, our experience coincides with that of Dr. Goodfellow regarding the benefit of chalybeates; we have also found the salts of iron and potass very useful.

In the appendix he quotes the processes for determining the quantity of urea in urine and blood; the method for detecting albumen in the urine; fat in the serum; the modes of testing for the amyloid degeneration; for detecting bile and sugar in urine, and the mode of examining for tube-casts, &c. Two plates of very good illustrations are added.

In concluding our remarks on this work we have again to express our very favourable opinion on its merits. To the junior practitioner, and to those who have not had time or opportunity of keeping pace with the progress of renal pathology and therapeutics, it will prove most valuable, as it will furnish them with a very good digest of all that is known of any practical value on the subject; and at same time form an excellent guide to enable them to discriminate the different forms of "Bright's disease" during life, and thereby put them in a position to conduct the treatment on the soundest principles. We hope the author will fulfil his intention, and at some future period "publish a more complete work on the diseases of the kidney."

SELECTIONS FROM MEDICAL JOURNALS.

I.—SPINA BIFIDA, TREATED BY IODINE; CURE BY ONE INJECTION. By DANIEL BRAINARD, M.D., Professor of Surgery in Rush Medical College, &c.

November 7, 1860, a girl three years old was brought to me to be treated for spina bifida. The child was intelligent, healthy, and well-formed in every respect excepting the tumour situated over the sacrum. This was eight inches in circumference at the base, about two and a half inches in height, conical, translucent, elastic, and covered with healthy skin, excepting a small point at the lower part where it was discoloured like the vestige of a nœvus. Below the tumour there was an umbilicated depression like a cicatrix adhering to the sacrum.

OPERATION—Nov. 10, 1860, assisted by Professors Ephraim, Ingalls, and Dr. Edwin Powell, the operation was performed as follows:—A small-sized hydrocele trocar was carried into the tumour at its base on the right side, and six ounces of fluid drawn off; while this was flowing pressure was made by an assistant, and, as the sac was emptied, the pulp of the thumb was pressed upon and partly into the opening in the spine which it exactly filled, so as to close it as perfectly as possible. Half an ounce of a solution (five grains iodine, fifteen grains iodide potass to the ounce distilled water) at the temperature of the body, was then injected through the canula, and after a few seconds allowed to flow out; distilled water at the temperature of the body was thrown in to wash out the iodine, and two ounces of the fluid first drawn from the sac and kept at the same temperature, were re-injected and the canula withdrawn. From movements of the child some bubbles of air passed into the sac, and as these could not readily be brought out they were left.

During the operation the child was kept under the influence of chloroform, of which it required a very unusual quantity, and, when this was finished, it remained fifteen minutes in a quiet sleep.

The puncture was dressed with a strip of isinglass plaster, and a compress supported by a band around the pelvis placed over it.

On awakening, the child made efforts to vomit and seemed to be severely nauseated for half an hour, when it fell into a light sleep. During the afternoon it vomited occasionally, refused food, asked for cold water, and urinated often.

11th—Has been restless during the night, probably from being kept lying on the face; pulse and heat of skin natural; puncture at eleven o'clock, twenty-four hours after the operation, found to be leaking; tumour tense; applied more perfect compression over it. During the day child drank freely of toast water, and in the afternoon fell into a free warm perspiration, which lasted two hours.

12th—Has slept well; asked for toasted bread twice, and ate it; seems perfectly well.

13th—Tumour tense, redness around the puncture; applied cloths dipped in warm water.

15th—Puncture leaking; passed a fine needle through the edges, tied a fine thread around it, in form of twisted suture.

17th—Tumour tense; tapped it on the left side near the base on sound skin with an exploring trocar, and drew off six ounces of slightly turbid fluid. Continued warm water applications.

19th—First puncture leaks slightly; needle withdrawn and compress supported by a truss placed over it.

20th—First puncture closed; child in its usual health, tumour flaccid, walls slightly firmer than before the operation. *Translucency quite gone.* Applied an india-rubber band around the pelvis so as to compress the sac.

25th—Tumour much diminished; walls firmer. Removed the band and substituted an umbilical truss, the pad of which was placed over the sac.

30th—Child in good health; tumour diminishing.

December 3rd—Tumour but imperfectly fluctuating and evidently filled with semi-solid contents.

From this time to December 31st the truss was kept applied with compresses of fine linen within the centre, so as to press the skin into the opening in the spine. It was taken off and replaced daily, so as to avoid excoriation. The child suffered no pain, was in perfect health, and played about as before the operation.

31st—The skin at the centre of the tumour is adherent to the opening in the spine, which is felt to be closed. A little fulness around the base at the upper part alone marks the vestige of the tumour. I advised the continued wearing of the truss unless it should excoriate, and the parents left for home.

February 10th—Child has remained well; no tendency to return in the tumour. Truss has been left off for several weeks at a time when the pressure produced restlessness.

The fluid first drawn from this tumour was perfectly limpid, had the peculiar odour of the cerebro-spinal fluid, and was very slightly albuminous.

With the microscope only a few epithelial scales, and a trace of coagulated fibrin, could be detected. That drawn on the seventh day was turbid, and on cooling deposited a sediment of coagulated albumen and fibrin, with what appeared to me to be pus-globules here and there. These, if I was not mistaken in their character, must have come from the internal orifice of the puncture, which was still leaking.

REMARKS—This is the seventh case of spina bifida which I have treated by iodine injections. In no case have I seen it produce dangerous symptoms. It is the third unaccompanied by hydrocephalus; all these three have been perfectly and permanently cured—one with thirteen injections, one with two, and the last with one. In the last two, means were taken to prevent the passage of the solution into the spinal canal. In one, the tumour being pediculated, this was easily done; in the other, the means above described were resorted to with satisfactory results. When this can be effected, the solution may be used strong, and washed out so as to render one or two operations sufficient. The object of reinjecting some of the fluid in the case above reported, was to enable a band around the pelvis to effect some pressure on the cord.

The operation is so delicate that it is not easy in any case to fulfil all the requisite conditions. Thus, in the above case, the walls were so thin at the point of puncture that it did not close for ten days, constituting a source of danger. Some bubbles of air also passed in, to which too much consequence need not be attached, as no harm resulted; but it would be preferable to avoid such an occurrence.

Applications of collodion for the cure of spina bifida have been recently suggested. When the walls are thick and firm this may be safe, and will be as serviceable as other forms of compression. When the covering is thin, it is dangerous. Dr. James Gow reported, in the *Chicago Medical Journal* for November, 1860, a case where it caused ulceration and rupture. Professor Gross reports in the *North American Medico-Chirurgical Review* for November, 1860, a

case of this malformation, treated by injection of iodine. "The tumour was thoroughly painted over with collodion." The tumour opened (not at the point of puncture) on the second day, and on the sixth it "burst completely." Although the covering is stated to have been "on the point of bursting" before the operation, it seems probable that the collodion hastened if it did not cause the rupture.

In another case by Professor Gross (*ib.*), where about ten injections were used in eight weeks, the collodion was kept applied, and the tumour burst, causing death. Concerning this case, there are two points deserving notice:—

1. As there is no reason to suppose that the rupture was caused by the injections, it must be attributed to the collodion.

2. As the tumour was found on dissection to be "about one-third obliterated by coagulable lymph," the inquiry naturally suggests itself, whether one or two injections would not have been sufficient. A certain thickening of the walls has indicated to me that further injections were unnecessary; and the loss of translucency, or the turbid appearance of the fluid withdrawn, are indications of a change in the structure and action of the lining membrane, sufficient, with judicious pressure, to effect a cure.

As these cases of Professor Gross, taken without detail, are calculated to discourage the trial, it may be well to note that, although about ten injections were made so as to pass in some degree into the spinal canal, "the child early suffered from the convulsions after the operations, and they always readily yielded to a dose of castor oil."

I regard both the cases of Professor Gross as tending to show the safety and efficiency of this method of treatment, as in both plastic lymph was deposited within the sac without any dangerous symptoms attributed to the operation having occurred.

The manner in which collodion acts in producing ulceration is threefold:—
1. By vesication. 2. By expelling the blood from the thin walls. 3. By increasing the tension of the walls of the sac, which it does by diminishing its size.—*American Journal of Medical Sciences.*

II.—RECTO-VESICAL LITHOTOMY—BOZEMAN'S BUTTON SUTURE.

By J. F. NOYES, M.D., WATERVILLE, ME.

History shows that this operation (recto-vesical) dates from about the beginning of the present century. M. Sanson, in an inaugural thesis, first described the operation. Of the several methods therein laid down, M. Maisonneuve, in later years, made an important modification. These are fully described in systematic works on surgery, and call for no special mention from us here.

From the frequent evil results and ill success consequent upon the escape of stercoraceous matter into the bladder, and infiltration of urine preventing the cicatrization and healing of the cut, the operation became obsolete.

The discovery of the "silver wire suture," the wonderful success consequent upon its use in treating vesico-vaginal fistulæ in the hands of Dr. Sims and others, led naturally to a trial of it in this (the recto-vesical) operation. Accordingly, we find that in 1859 Dr. S. first applied it in the case of a recto-vesical operation performed by Dr. Bauer of Brooklyn. The wound healed by the first intention, in eight days, and its employment here was announced as the beginning of a new era in lithotomy. At that time we had our second patient, on whom we were about to operate for stone, and we decided to give it a trial.

The following is a report of the case:—A Mr. Jones of Corinna, Me., aged 39, of a healthy and vigorous constitution, for more than three years had been troubled with more or less pain and distress in the bladder, particularly when urinating. At the time of our first visit he had been confined to the room, under the care of a physician for several months, and was in a very emaciated and feeble state from protracted suffering, and from a large open abscess under the throat.

Introducing carefully a sound, I detected a stone lying loosely in the bladder. Upon further examination, it was thought best to postpone the proposed operation for at least one month, with the hope that he would then be in a more favourable condition. At the expiration of this time, however, our patient was scarcely any better, and there appeared nothing to be gained by delay.

October 21st, 1859—In the presence of a number of medical gentlemen, and with the assistance of Drs. Benson of Newport, and Wilson of Dexter, the patient being fully etherized, and placed in a convenient position on the left side, with the thighs flexed, I proceeded with the operation as follows:—Dilating the rectum with an ordinary speculum (not having Sims' at hand, which I very much regretted), the bladder being partly filled with lukewarm water, and a sound introduced and held by an assistant, I then made out with my finger the situation and limits of the prostate gland. With a two-edged scalpel, a cut was made through its central portion and bilaterally enlarged sufficiently to enable me to introduce my index finger, when it was dilated a little. The forceps were now introduced, the stone seized and extracted without much difficulty.

The stone is of an oval form, quite hard, with a rough surface, and measures, in its long diameter, an inch and a half; in its short diameter, three quarters of an inch. I have not yet had it analyzed, so as to know of what it is composed. Six silver-wire sutures were required to bring together and coaptate the edges of the wound. Upon these I adjusted "Bozeman's button," in the manner I had seen him apply it in the case of a large vesico-vaginal fistula operated upon by him in the fall of 1858 at the Hotel Dieu, which united by the first intention, and was cured in eight days.

On the twelfth day after the operation I removed the apparatus, when it was found united by the first intention, save a small place in the centre. I could not be surprised at this slight failure, in view of the very unfavourable state of our patient at the time. A few touches, however, with nitrate of silver, from my colleague, Dr. Benson, under whose care the patient was left during my absence, was all that was necessary, as it closed by granulation in a short time. It was observed that a sliding down of a fold of the rectum over this small opening seemed to guard against the escape of anything into the bladder while it was healing.

Our patient was obliged to keep his bed for a long time, on account of a large bed-sore on one of his hips, which was very slow in healing. But at the end of three months he was weighed, and it was found that he had gained about seventy pounds, making his entire weight two hundred. Both of these patients (the first operated upon by the lateral operation more than three years since) are now in good health.

It may be important to state in closing this report, that both of them had used, for a number of years, water from wells containing lime.—*Boston Medical and Surgical Journal*.

III.—THE PATHOLOGY OF SYPHILIS.

Dr. Alph. Guérin, having obtained permission from the Administration of hospitals to deliver at the female venereal hospital (Lourcine) a course of

lectures on syphilis, describes as follows the present state of science on the subject:—

The pathology of syphilis was in a state of extreme confusion when Mr. Ricord first addressed himself to the subject. Gonorrhœa was considered capable of inducing secondaries; mercury and all its compounds were indiscriminately prescribed for the various forms of the disease, while the school founded by Broussais assumed syphilis to be of an inflammatory or phlogistic nature, requiring depletion and debilitating methods of treatment. A novel doctrine, propounding a new criterion, inoculation, was therefore a boon, and although many of Mr. Ricord's original views must now be considered untenable, he undoubtedly has rendered eminent service by the order he has introduced in the classification of the symptoms, by the mode in which he has traced their connection with each other, and by his researches on inoculation, although the results of this procedure must now be interpreted in an entirely different manner from that with which they were first presented to the profession.

A striking fact is now fully demonstrated, viz., that indurated chancre is never, and soft chancre always inoculable to the person who bears it. Indurated chancre moreover is invariably, as Rollet has abundantly proved, preceded by a more or less protracted incubation. This is not the case in soft chancre. These distinguishing characteristics, combined with the circumstance now universally admitted by syphilographers, that each form of chancre arises from the contagion of a sore of a similar description, and cannot be generated by any other, demonstrate, as Mr. Bassereau has shown, the existence of a separate virus for each form. The inoculation of chancre has now therefore a totally different significance from that it formerly implied; when a primary sore is inoculable to the patient himself, he may be safely informed that his disease is purely local, and requires no mercurial treatment; when, on the contrary, inoculation does not succeed in reproducing the chancre, the failure denotes that the constitution of the subject is already contaminated, that secondaries are impending, and that mercury must be exhibited. Thus, to derive an illustration from another description of inoculation, vaccination invariably fails after pure vaccinal pustules have been evolved. With regard to the treatment of chancre, Mr. Guérin chiefly relies on aromatic wine, and prohibits mercurial or unctuous dressings.

The Professor describes three kinds of bubo: 1, the inflammatory; 2, the virulent, which secretes inoculable matter; and 3, the hard glandular enlargements coincided with indurated chancre, and situated not in the inguinal fold only, but in the neck, the axilla, &c. With regard to treatment, Mr. A. Guérin recommends, especially in the second form, even when suppuration is present, and fluctuation is distinct, and the abscess seems on the eve of bursting, the application of several blisters in succession. In most cases, says he, this method causes the reabsorption of the contents of the tumour, and we must acknowledge that in the numerous visits we paid to his wards containing upwards of ninety beds, we did not meet with a single instance of ulcerated bubo.

For many years Mr. Ricord has professed that when once a subject has contracted an indurated chancre, and been affected with secondary symptoms, he enjoys immunity from future infection; that is to say, that any chancres he may subsequently be so unfortunate as to take by contagion will be soft, and therefore produce merely local injuries. This view is, in a great measure, adopted by Dr. Guérin. In syphilis, however, as in small-pox, a second infection, after the poison has been in the course of years eradicated from the system, does not appear impossible.

Mr. Guérin expatiates at some length on the description of the mucous papulæ or flat condyloma in the various regions of the body, and laid much stress on the very different appearances it presents in each. This symptom is usually

the first manifestation of syphilis observable in women, and its contagious character now seems established beyond contradiction by the experimental inquiries instituted by Waller of Prague, and W. Wallace of Dublin.—*Journal of Practical Medicine and Surgery.*

IV.—A NEW OPERATION FOR THE RADICAL CURE OF HERNIA.

By J. J. CHISHOLM, M.D.

Few subjects have engaged so much attention within the last few years, both among European and American surgeons, as this of the "Radical Cure of Hernia." This is due, doubtless, both to the exceeding frequency of this disagreeable condition, as also to the various methods recently proposed for effecting such a cure. Gerdy, among modern surgeons, led the way; Wützer, Rothmund, Schuh, Langenbeck, and others, improved upon his method. All these operations propose to effect the cure by inserting a plug into the inguinal canal, and by the irritation thus produced, to excite sufficient inflammation in its coverings, to obstruct, if not to occlude this canal. Each new operation, in its turn, claimed the most splendid results. The successful cases were published by hundreds; but the thousands of failures were unheard of. This was doubtless owing to the fact, that these results were always published soon after the operation; too early to decide positively whether they would be permanent or not, for the deposit of lymph forming the adhesive bands is very apt to be absorbed, and upon any unusual muscular exertion or "strain," the hernial protrusion reappears, very much to the dismay both of patient and surgeon. Indeed, we may not venture too far in asserting, that the successful cures are, perhaps, generally, cases where the lesion is of recent occurrence, or in individuals but little exposed to undue muscular exertion, and who would find sufficient relief and protection from a good truss. Wützer's operation is applicable only in recent, small, oblique herniæ; and where we can select our cases, we perhaps may be rewarded with a success of fifty per cent.; but in average cases, failure is the rule, and a radical cure the exception. Nor is this all; for in many cases of failure after this operation, we have the canal more dilated than before, and hence a greater hernial protrusion. This operation, and the principle upon which it is based, are now generally discarded, owing to the fact that Mr. Wood of London, some two years ago, discovered and published a new and far superior method for effecting the same end. He makes a small subcutaneous incision in the upper and anterior portion of the scrotum, dissects the fascia, and invaginates it into the inguinal canal, then passing a needle with thick thread through three points in the canal, viz., the conjoined tendon, the triangular fascia, and the external pillar of the ring close to Poupart's ligament. The ends of the ligature are left in the two former punctures, and a central loop in the latter, passing through the pillars of the external ring, and through the same opening in the skin of the groin. A compress of glass or wood is then tied firmly upon the axis of the canal, by passing the ends of the ligature through the loop, and tying over the compress. The advantages of this operation over all its predecessors are obvious, and its successes in a high degree encouraging. It is adapted to inguinal herniæ of every variety, large and small, old and recent, direct or oblique. Even in case of failure, the patient would be in a better condition than before the operation.

Dr. Chisholm, Professor of Surgery in the Medical College of South Carolina, after seeing Dr. Wood operate in June, 1859, thinking that the incision in the skin was unnecessary (as the invagination of the fascia alone did not obviate the objection Mr. Wood expected to meet by this process, viz., the prevention

of any dragging upon the invaginated scrotum), modified that operation, first, by invaginating without incision, as in Wutzer's; and secondly, by only making two punctures instead of three, Dr. Chisholm believed that a single loop passed from without through the two columns would be sufficient to obliterate the ring, and keep the columns in apposition, until the lymph effused in the site of the thread would cause adhesion, and permanent obliteration of the ring, restoring the external oblique to its primitive condition, before its fibres had been forced asunder by the protruding body. The first case operated upon in this manner was in November 18, 1859, and the operation has been often since repeated, both by himself and others in this city, with the best results. Dr. Chisholm published this operation in the *Charleston Medical Journal* for May, 1860. In the *London Medical Times and Gazette* for February 4, 1860, two cases are reported by Messrs. Curling and Ferguson, adopting nearly the same modification upon Wood as this of Dr. Chisholm. The honour of priority, however, belongs to the American surgeon, Dr. C. having operated November 18; Mr. Curling, December 1; and Mr. Ferguson, December 17, 1860. Other modifications of this operation have since been suggested, but of minor importance, such as the different curves of the needles employed, the clamp upon which the ligature is fastened externally, as also the material used for the suture. Although Wood's operation is a great improvement upon all previously devised, it still has its disadvantages. Even if we overlook the incision which complicates the operation, and would deter many from availing themselves of its advantages, we still have the length of time necessary to keep the patient in bed—not less than from twenty to thirty days; after that, a truss has to be worn for a considerable period, to counteract any undue pressure upon the recent inflammatory agglutinations. The suppuration from the sutures, and the continuous pain connected with the inflammation, are likewise disadvantages. These, and other considerations, have induced Dr. Chisholm not to rest satisfied with the successes gained, even by his modification of Wood's operation, but to add a still greater improvement, simplifying the entire procedure, and obviating nearly all the objections which have been, or may be urged to Wood's.

The new operation is as follows:—

The scrotum having been invaginated upon the finger, as the only mode of guiding the needle in its passage—a long strong curved needle, fixed firmly in a handle, and armed with silver wire; guided by the finger, transfixes the scrotum at the apex of the invaginated portion, passes through the internal column, and appears through the skin of the abdomen, when one end of the wire is drawn out. The point of the needle is then drawn backwards, and disappears again in the canal. Its direction is then changed. Whilst still imbedded in the scrotum, and guided upon the finger, its point is made to traverse the external column of the ring near Poupart's ligament, lifting the skin of the abdomen. By gliding the skin upon the needle, the point appears through the small puncture made by the first passage of the needle; when the other end of the wire is seized, the needle is unarmed and withdrawn through the scrotum.

The finger is now removed from the canal, and the two ends of the wire being drawn upon the loop dissect the cellular tissue up to the columns, which it hugs closely. By twisting the two ends of the wire the columns are felt approaching, until they are brought in such close apposition as to allow nothing to pass between them—the spermatic cord, in its exit, filling up all the available space remaining of the ring. When the ring is felt closed, the twisted wire is drawn firmly outwards, and clipped off as close as possible to the skin, so that when the traction on the skin of the abdomen is removed, the gliding back of the integuments to their normal position conceals completely the ends of the small loop of silver wire. The scrotum has already fallen back to its pendent position, and the only trace of an operation having been performed is in the two small punctures, one in the scrotum, the other in the abdomen, which require a careful

search to find them, and which will heal up in a few hours, hermetically incarcinating the silver wire.

A moderate inflammation follows this operation, without much swelling or pain, and without any fear of suppuration. The wire is soon imbedded in a lymphic deposit, which will not only inclose it, thus isolating it from the tissues, but at the same time agglutinates the columns together as an additional security to the success of the operation. The patient is kept quiet in bed for four or five days, until the inflammatory stage passes; opium having been given to insure rest, and prevent any action on the bowels. When the inflammatory stage has passed, a cathartic is administered, and the patient can quit the bed, and in a few days resume his occupations. The silver wire remains as a permanent application. An essential element in the success of the operation, is that the loop encircle the columns of the ring near their points of attachment to the pelvis; otherwise the columns cannot be approached, the ring remains open, and the results can only be negative. If this step of the operation be carefully followed, a radical cure may nearly be guaranteed.

The advantages of the operation are as follows:—The patient is not detained in bed on his back for three or four weeks, as in Wood's or in Wutzer's. No excess of inflammation is to be apprehended. No subsequent use of a truss is required, and there is no fear of a return of the rupture from the giving way of the recently formed but still delicate adhesion, through any undue muscular effort on the part of the patient, for the reliance is upon the silver bond—the surgeon having provided his patient with a never-failing silver truss. The operation is applicable to herniæ of every character. When the protrusion is large, and the ring voluminous, several points of suture might be applied through the same puncture in the scrotum, and skin of abdomen, taking advantage of the facility of gliding these integuments over any portion of the external abdominal ring.

The objections which may be urged against this operation will probably be, that the silver wire will always act as a foreign substance; but from the experience of Drs. Sims, Simpson, Moffatt, and others, we may assume that this is not the case, and from experience in its application in hernial operations, we know that it can remain harmlessly imbedded in the tissues for any length of time. Of course flax or silk sutures cannot be used in this subcutaneous operation. Another objection perhaps may be, that the cut-off twist of the wire will irritate and ulcerate the skin. But this has not been found so, for the skin here is very loose, and therefore not so liable to be injured by a small foreign body beneath it; and we have every reason to believe that a deposit will soon encase it, and render it permanently innocuous. These views were first practically carried out upon the living subject, November 17, 1860, at the surgical clinique, in the presence of the class and a number of professional gentlemen. Three cases have since been operated upon. And as herniæ are exceedingly common lesions among the labouring negro population of the southern states, and as the carelessness of this class of people renders the advantages of a truss nugatory, ample opportunity will be afforded of testing in time the validity and superiority of this operation over all other modes of radically curing inguinal hernia.—*American Medical Times*.

V.—PROFESSOR LANGENBECK ON VENOUS HÆMORRHAGE.

The last number of the "Archives de Médecine" contains an interesting exposition of Professor Langenbeck's ideas on the treatment of venous hæmorrhage.

M. Langenbeck objects to the use of styptics, the best of which (sesquichloride of iron) he considers dangerous on account of the extensive extravasation of

blood, and subsequent irritation to which it gives rise. In reiterated and obstinate hæmorrhage from numerous small veins, he has recourse, in preference, to the actual cautery as the agent best calculated to induce the formation of solid coagula, and therefore to prevent puriform absorption. When blood escapes from a large vein, he conceives digital pressure of its orifice to be in most instances sufficient, when the hæmorrhage occurs in one of the limbs. If one of the jugulars is wounded, both orifices should be closed by compression; that situated nearest to the trunk to prevent the penetration of air, and the peripheric extremity to check the escape of blood. In this case, unless the wound is very small, digital pressure can only be viewed as a provisional resource, and should be replaced by strips of adhesive plaster so arranged as to bring into close union the edges of the external aperture, and to press in a uniform manner without including the neck in the bands, in order to avoid interfering with the course of the blood through the jugular of the other side. A roller may also be used with advantage to restrain hæmorrhage from the veins of the limbs. When the injured vessel lies at the bottom of a deep wound, the author applies a piece of linen smeared over with cerate, fills the part with lint, and unites the lips of the division with strips of adhesive plaster.

In addition to these measures, ligature of the vein may also be resorted to. In general, ligature of the peripheric extremity is alone necessary; but in some cases, a ligature should be applied to both divided ends of the vein, when the blood-vessel receives a large branch at a short distance from the wound. It is occasionally requisite to secure provisionally subcutaneous veins, which may be considerably distended during the removal of large tumours, in tracheotomy, &c., to avoid excessive hæmorrhage. In this case M. Langenbeck always divides the vein across, between two ligatures, which he removes at the conclusion of the operation. He lays much stress on this practice, because the circumstances which lead to the introduction of air into the veins are yet but imperfectly known. When the external jugular is wounded, its central extremity should be secured, a preliminary precaution which should always be taken before any operation in which it may be exposed to injury. In every other case, ligature of veins should be avoided, especially in large hospitals, where thrombus and puriform absorption are more likely to occur. In these ligatures, the internal membrane of the vessel does not give way as in arteries, and the circulation may at a future day be re-established.

To conclude, M. Langenbeck does not hesitate to recommend, for the extremities, ligature of the artery, when pressure does not succeed in checking the escape of blood from a large vein, and he adduces a case in point in which the life of the patient was thus saved. During the removal of a sarcomatous growth of the thigh, he had wounded the femoral vein, which was dilated and friable. A fearful hæmorrhage was the consequence; the femoral artery which had been exposed during the operation was at once taken up, the loss of blood was arrested immediately, and the patient recovered. M. Langenbeck conceives that simultaneous ligature of the vein would, in such cases, be unnecessary, and might even be dangerous, on account of the thrombus which might possibly result.—*Journal of Practical Medicine and Surgery.*

VI.—HÆMATURIA.

Professor Oppolzer says: In hæmaturia it is often difficult to determine whether the blood comes from the bladder or the kidney, inasmuch as, for example, we cannot with certainty infer the existence of renal disease from the presence of exudation-cylinders which microscopically exhibit the form of Bellini's tubes, for ordinary blood-coagula may accidentally present this form;

therefore renal disease is to be assumed to be the cause of the hæmorrhage only when the exudation-cylinders contain but few blood corpuscles on their surface. The rapidity of the deposition of the sediment in the urine is by no means decisive in favour of vesical hæmorrhage, as the duration of the contact of the urine with the blood is not known. The author considers the full investigation of the urine alone to approximate to a solution of the question. This in renal diseases exhibits decrease of the urea, uric acid, and colouring matters (particularly of Helier's urophæin), increase of the urinary indigo, and an almost constant accession of albumen. If, on the other hand, symptoms of a vesical affection are observed, there is the possibility of vesical hæmorrhage, especially if the urine comes in a decomposed state from the bladder. The causes of vesical hæmorrhages are manifold: stagnation of the urine, spasm of the bladder, paralysis, injuries of the vesical region and of the bladder by foreign bodies (calculi), cancer; but certain medicines (cantharides) and changes of the system at large (scurvy) may also give rise to bloody urine. The same thing, moreover, happens spontaneously, as in the inhabitants of the Isles of France and Bourbon; or it is, as in Egypt, also caused by the hematobium, which may also give the impulse to the formation of urinary calculi. In all these cases the seat of the hæmorrhage must be deduced from the symptomatology of the anomaly in question. Renal hæmorrhages occur in morbus Brightii; and inflammations of the kidney are connected with renal calculi, cancer, and tubercle of the kidney, but are observed also in cardiac diseases, compression and obturation of the renal veins, in scurvy, and strongylus gigas. The treatment of hæmaturia consists in the treatment of the disease on which it depends.—"Vierteljahrsschrift für die praktische Heilkunde," 1861; Band, 69; Anelekten, p. 62.—*Medical Circular*.

VII.—HISTORY OF A CASE OF THE DRACUNCULUS OR GUINEA WORM (*FILARIA MEDINENSIS*). By C. E. SEVERANCE, M.D.

James H. Harvey, nativity, N.Y., aged 32 years, was admitted into the hospital, June 11, 1860. The right ankle was considerably swollen, extending on the external side nearly to the knee, quite dark coloured, and attended with intolerable itching throughout the day. From six or seven o'clock in the evening until daylight he was every night kept awake, and sometimes almost frantic, from the jerking pain, as he expressed it, which seemed as though the bone was being gnawed off.

About four weeks previous to admission, while "before the mast" *en route* from New Orleans to New York, he first observed a swelling, which commenced below the external malleolus, attended with slight itching, which gradually increased and extended. June 17th, about two inches above the malleolus, a vesicle was punctured at the apex of what appeared to be a boil, when with the semi-transparent serous discharge, in quantity perhaps half a drachm, a dracunculus was observed, a loop of which protruded fully an inch. This was the sole cause of all his trouble. Dr. Livingstone's method was immediately resorted to, which is the roller compress and adhesive straps, and in forty-eight hours the head together with half a yard of the body was extracted. The patient being alone when the head appeared, was anxious to remove it himself, but making too great traction, he broke the body at the puncture, leaving a portion of unknown length hidden in the tissues. Inflammation and pain followed, increasing for several days. Lines of livid red, one-fourth of an inch wide, extended around and across the leg, and half the distance from the knee to the hip. These gradually subsided, and the swelling and pain diminished day by day, and, after considerable suppuration, the wound healed. August 1st, the patient was discharged cured.

The dracunculus, of the class entozoa and species *filaria medinensis*, is of that division in which is found a distinct intestinal canal, orifices at both ends, and traces of a nervous and muscular system. It is confined to tropical localities; seldom more than three yards long; and is found of diminished length according to its age. The female only has been discovered, which resembles a flattened cord one line in diameter, of a yellowish colour. The head is round, a little larger than the body, and armed with four straight pointed spines, by which it probably penetrates the tissues. It is found in the subcutaneous areolar tissue, chiefly of the ankles, feet, and legs; very seldom in the abdominal parietes or arms. Sometimes it appears as an endemic, attacking certain regiments in armies and sparing others.

Dr. Livingstone believes it mostly inhabits low marshy districts, following the rainy seasons; that other animals become infected only by coming in actual contact with it; hence the natives of Africa guard against wetting their feet, bathing in standing pools, or sleeping on the ground. As many as fifty have been found in one person; more than one, however, seldom occurs. It is viviparous, and the young are supposed to be impregnated before entering the flesh of other animals, and after attaining their full development attempt an escape to their native soil in which to deposit the germs, where the male constantly remains. When it is superficially situated its growth can be watched, which is quite rapid, oftentimes increasing more than an inch in twenty-four hours. When deep, it may remain a long time undiscovered, and be transported to foreign countries, without showing any symptoms of its presence. Before opening externally, a little boil may be observed, from which when it bursts, or is opened, the anterior part of the worm protrudes. Gentle and continued traction should be made, the compress rolled three or four times per day, and great care taken not to break it, for serious results will be most likely to follow such an accident.

Whether the germs ever live and mature in the flesh after the death of the parent from such accidents, without first being deposited in the ground, is doubtful, and in view of that, the above-named case is an interesting one to observe. This case is interesting also in showing how long a period this entozoon can remain burrowed in the tissues, without acting as a foreign body, it being full six years since the patient was in Mogador, Morocco, where he remained nearly a month travelling with uncovered feet, and sleeping on the ground. He returned to New York in August, 1854, and has since remained in the United States.—*American Medical Times*.

VIII.—SPONTANEOUS GENERATION.

The question of spontaneous generation is one of the subjects proposed for competition by the Institute, and communications are occasionally forwarded to that body by those who are likely to appear as candidates for the prize. Thus, at a recent meeting, M. Pasteur presented a work in which he endeavours to prove that all organized productions which appear in previously heated infusions, are due to the microscopic organized particles floating in the air. Another communication was also received from M. Jodin. This gentleman states that, on the 12th May, 1859, he took six glass tubes five inches long, and of a diameter of from seven to eleven lines, and hermetically closed at one end. Each tube received equal parts of crystallized sulphate of soda and water to about one-fourth of their capacity. Three, moreover, received a weight of sugar-candy equal to one-third of the sulphate of soda employed. All the tubes were then heated in a water bath, to liquefy their contents, corked and kept inside a press, in a vertical position. At the expiration of a twelvemonth,

M. Jodin examined the tubes, and found that the contents of all those which had only received sulphate of soda were totally crystallized, while those which also contained sugar had remained liquid; but this liquid was covered with a pellicle formed by a well-developed mycodermic vegetation, possessing the peculiar greenish hue of the spores of the *Penicillium glaucum*. Five minutes after the corks had been carefully removed the liquid crystallized in one solid mass. Hence it appears that the germs of the genus *Mucedinæ* (or mildew) may be developed and organized in contact with a solution of sulphate of soda, the particles of which are arranged in that peculiar way which constitutes a state of excessive saturation.—*Galvani*.

IX.—SUCCESSFUL CASES OF CÆSARIAN SECTION.

1. By I. J. NEWTON, M.D., Hamburg, Arkansas. Reported by J. M. OWENS, M.D.

Inasmuch as the Cæsarian section is an operation in itself so simple, and one so generally known to the profession, I deem it unnecessary to report the case under consideration in detail; but, however simple the operation, however well known to the profession, it is nevertheless an operation that has ever been attended with fearful mortality; and when the mother and child are both saved, as in this case, it can but be regarded as pre-eminently successful.

On the morning of the 7th January, I was called to see Ann, a negress, belonging to Col. Waddell of Hamburg. I found the girl in labour with her first child. After an examination, I found that it would be impossible to deliver the woman *per vias naturales*, owing to pelvic deformity. The head could not even enter the pelvis; it could not pass the sacral promontory. The deformity resulted mainly from curvature of the lumbar vertebrae, the promontory of the sacrum jutting into the basin of the pelvis, and partly from depression of the pubic bones, making the space between those bones so small as altogether, in my judgment, to preclude the idea of craniotomy or evisceration.

Viewing the case in this light, I requested Drs. Dickinson, Wilson, and Owens to see the case, who, after an examination, agreed with me that the Cæsarian section was the only method that afforded any chance or hope for the successful delivery of the mother, or preservation of the child.

To make this report a brief one as it should be, I will only add, that the operation was performed in the usual way, and the woman was delivered of a male child, at full term, in a state of asphyxia; the child, however, revived after an hour's perseverance in Marshall Hall's method of resuscitation. The after-treatment consisted of opium in full doses, for several days, with mercury in sufficient quantities to produce slight ptyalism.

The mother and child are well, and there now remains no visible trace to show that the child is a Julius Cæsar or Æsculapius, or that the mother has been the subject of hysterotomy.—*New Orleans Med. and Surg. Journal*.

2. By Dr. GODEFROY, Professor of Anatomy in the Medical School of Rennes.

Mme. F., a rachitic woman, only 1 mètre 24 millimètres in height (40·31 inches), and with the pelvic brim greatly contracted (2·79 inches in its antero-posterior diameter), was twice delivered by induction of premature labour at the commencement of the seventh month, once by Dr. Godefroy, and once by Dr. Rouault. On the occasion of her third pregnancy, she allowed pregnancy to advance to the end of the seventh month, and when Dr. Rouault hesitated to decide whether or not delivery was possible, she fell down stairs and labour came on. After the membranes gave way, an arm came down, and podalic version was unsuccessfully

attempted by Drs. Rouault and Pitois. On the evening of the same day Dr. Godefroy saw the patient in consultation, in order that it might be determined whether embryotomy or the Cæsarian section should be resorted to. The latter being decided on, Dr. Godefroy operated without delay in the manner recommended by Mauriceau. On cutting through the uterus he came directly on the attachment of the placenta, which caused considerable hæmorrhage until the fœtus and placenta were removed, when it was easily checked by cold water. The recovery was a little protracted, owing to the persistence of a fistulous communication in the lower part of the abdominal incision; but on the 20th day she got up, and was walking out within six weeks of the operation.

In his remarks on this case, Dr. Godefroy gives the details of three cases in which he operated with a fatal result. In one case he operated before the membranes had ruptured, and he is of opinion that the result would have been successful had not the patient been seized with severe vomiting, during which the abdominal wound was torn open, and the intestines protruded. She vomited several lumbrici, and after death more than a hundred were found in the small intestines. In the other two cases the operation was delayed.

In concluding, Dr. Godefroy compares the practice of Germany, France, and England. He says:—

“The Germans, who operate very early, save many women.

“The French, who temporize more, save a smaller number.

“The English, who never operate but in the last extremity, lose almost all.”

—*Gazette des Hôpitaux*.

X.—RUPTURE OF UTERUS; RECOVERY.

By G. T. ELLIOT, Jun., M.D., New York.

On the 25th November, 1860, I was called by Dr. Slevin to see Mrs. M. in her second labour. The first had been severe, but terminated naturally. She had suffered for eighteen hours, when she complained of a sharp, agonizing pain in the left iliac region, and the contractions ceased. Before this the brow, tips of the fingers, and the funis had been recognized as presenting, but they have now receded. She was weak, and was vomiting a clear green fluid; pulse 130, and feeble. Within the cervix, to the left, was a longitudinal fissure, which did not involve the entire thickness of the cervix. It was decided to turn; the patient took some stimulus, and the operation was completed, the delivery of the head requiring its being broken up, as locking occurred. After the delivery, the fingers could be passed through the rent so as to feel a loop of intestine and the peritoneal coat of the abdominal wall. Slight hæmorrhage took place and contraction ensued, which was aided by ergot and ice in the vagina. The ergot was vomited almost immediately. The patient was placed in bed, and care taken to procure reaction. The vomiting was persistent for two days, when she began to improve, and by the latter part of December was out walking. The treatment was solely sedative and stimulant.”—*Am. Med. Times*.

XI.—NEURALGIA: THE ENDERMIC METHOD; INSERTION OF MEDICATED CYLINDERS INTO THE TISSUES.

We are still very far from the time when hypodermic injections will be commonly resorted to in practice. Wood's method is doubtless highly valuable, but its general adoption will be retarded by the necessity of a special apparatus for the performance of his operation. It is therefore the duty of the press, until Pravaz's syringe becomes more simple and less costly, to indicate

slender than a knitting needle, divided into fragments of one and a half line in length, and dried at a gentle heat. If the operations have been carefully conducted, twenty-five small cylinders or tacks will thus have been made, containing each one twenty-fifth of a grain of sulphate of atropia. The formula is the same for muriate of morphia, sulphate of strychnia, veratrine, digitaline, aconitine and delphine; the quantity of sugar and gum being increased in proportion to the amount of the active agent it is intended each cylinder should contain.

A needle is also requisite; that used by Mr. Lafargue is a common small darning needle, the point of which can be shaped by any cutler like the extremity of a trochar.

Such is the instrumental apparatus; it is used as follows—The skin over the painful region should be stretched between the fore-finger and thumb of the left hand, and the needle held in the right should be rapidly inserted in a slanting direction beneath the derm to a depth of about an inch and a half. The needle is then withdrawn, and either with the fingers or with a forceps, a medicated cylinder is immediately introduced in its place. When the tack, less voluminous than the needle has entirely disappeared in the wound, it is secured by a shield of collodion, adhesive plaster, a postage stamp, or a mere wafer. Thus incarcerated, the cylinder soon dissolves, and the physiological and curative action of the remedial agent resorted to becomes apparent in less than a quarter of an hour.

When the neuralgia occupies the course of a large nerve, such as the sciatic, Mr. Lafargue does not confine himself to one puncture, but inserts several cylinders into the painful points, which Valleix has indicated. The poisonous action of atropia must, however, not be lost sight of, and in order to obviate its injurious effects, one cylinder may be divided into several fragments, each of which can be separately inserted. This is a minor detail, which the sagacity of the practitioner will readily appreciate and modify according to circumstances.—*Journal of Practical Medicine and Surgery.*

XII.—MR. T. HERPIN'S PRESCRIPTION FOR HEMORRHOIDS.

After many unsuccessful attempts, Mr. T. Herpin has at last compounded a pomade which facilitates the reduction, retention, and resolution of dry hemorrhoids. The following are its ingredients:—

R. Tannin..... 1 to 3 scruples.
Cold cream $\frac{1}{2}$ oz.

Mix with care.

The tumours smeared over with this ointment are reduced without delay. The patient is then required to rest for some time in a sitting, or preferably in a horizontal attitude. If the motions are painful, and the piles large or numerous, the dose of tannin should not exceed one scruple; in opposite cases, it should reach three scruples, and two only in intermediate circumstances. If the piles are in a state of considerable irritation, the pomade should be weaker, or even replaced by simple cold cream.

To relieve constipation, Mr. Herpin has often resorted with benefit to the following powder:—

R. Pulv. sulphuris loti }
Magnesiæ calcin..... } *ââ equal parts.*
Saccholactin }

Misce optimè.

A tea-spoonful or thereabout should be daily exhibited, so as to secure one easy

passage, or at most two. This medicine may be taken for several successive days, or every second day, but for one week only.

Although he has long been in the habit of prescribing the solanaceæ in various doses, Mr. T. Herpin has never found either belladonna or henbane possessed of the mild laxative properties they have been invested with by Messrs. Trouseau and Pidoux. Mr. Herpin lays much stress on the necessity of contending with flatulent dyspepsia, a frequent cause of constipation. For this purpose, he recommends Vichy water or bicarbonate of soda; he prohibits acids, fatty substances, vegetables covered with a cuticle, coffee, and spirits. Whether dyspepsia be present or not, says Mr. Herpin, nothing is better calculated to establish regularity of the intestinal functions than red fruit, and especially strawberries. The latter should be taken every day at one meal at least. In July and August they may be replaced by peaches, juicy pears, and in winter by soft dates, which Mr. Herpin has found most beneficial in cases of habitual costiveness.—*Bulletin de Thérapeutique*.

XIII.—ESCHAROTIC FOR TOOTHACHE.

Du Calon, physician in chief of the civil hospitals of Toulon, communicates in the *Gazette des Hôpitaux* the formula of an escharotic which would never fail in producing the speedy and certain effects in vain sought for from the thousand and one specifics extolled for the relief of toothache, and of the consequent neuralgic pains. The hollow tooth should be cleaned and cauterized with a cotton plug impregnated with

Acidi nitrici diluti..... 1 dr.
Morphiæ acetatis..... 1 gr.

As soon as the caustic has penetrated into the decayed cavity, the pain departs for two or three weeks, and sometimes for ever; indeed, observation shows that in some cases incipient caries is entirely checked after one cauterization. When the pain has subsided, it is however prudent to fill the tooth.—*Journal of Practical Medicine and Surgery*.

MEDICAL INTELLIGENCE.

1. *Appointment of Dr. John Couper as Assistant Surgeon to the London Hospital*.—We are glad to learn that Dr. John Couper, son of the late professor of materia medica in the University of Glasgow, has been appointed assistant surgeon to the London Hospital. Dr. Couper has held for some years the post of demonstrator of anatomy at the Medical School of the Hospital.

2. *New Rule of the Royal Medico-Chirurgical Society of London*.—This excellent and influential society shows symptoms of borrowing a leaf from the book of our French brethren, as the following deliverance of the council will show. It is certainly a move in the right direction:—

“That the council from time to time appoint committees from amongst the fellows of the society, for the purpose of investigating questions of scientific medical interest; and that such investigations be carried on at the expense of the society, and that the report of such committees be published in the ‘Proceedings’ or ‘Transactions,’ as the council may think fit.”

2. *Death of Professor Quekett, F.R.S.*—The profession has sustained no ordinary loss in the death of Professor Quekett, which occurred on the 20th of August. His name has long been known as one of the most accomplished

microscopists of the day, and of this he has left abundant proof in the Museum of the Royal College of Surgeons, where he succeeded Owen as conservator. His name will long live in the memory of his professional brethren.

4. *Quarterly Report of the State of Disease in the Glasgow Royal Infirmary for the Quarter ending 24th September, 1861.*—The number of patients remaining in the medical and surgical wards on the 30th June was 272, the number admitted since has been 680, so that the total number treated has been 952; of these 659 have been dismissed cured or relieved, 78 have died, and 215 remained on the 24th September.

In the fever wards, the number remaining on the 30th June was 35; the number admitted since has been 96, and the total number treated has been 131; of these 88 have been dismissed cured, 19 have died, and 24 remained on the 24th September.

The number of accidents admitted has been 136.

LETTER FROM DR. BELL TO DR. MURCHISON.

GLASGOW, September 11th, 1861.

DEAR SIR,

I have learned from a mutual friend that you consider my comments on your Essays as tantamount to an accusation against your literary honour. This I regret exceedingly. I have read over my article, and whilst I admit that the criticisms are severe, yet I do not think that they will justify your conclusion. But, be this as it may, I beg to assure you that I had not the slightest intention either to impugn your honesty, or to ascribe unworthy motives to you.

Though you and I hold different views regarding fever, yet I have no hesitation in ascribing to you, that which I claim for myself, an honest desire to arrive at the truth.

I am exceedingly glad that the matter has been brought under my notice; and in order to prevent any of the readers of my paper entertaining the same interpretation which you have put on my remarks, I shall have much pleasure in inserting this note in the October number of the *Glasgow Medical Journal*.

Believe me, DEAR SIR,

Yours truly,

JOSEPH BELL.

DR. MURCHISON, LONDON.

BOOKS RECEIVED.

A Treatise on the Surgical Diseases of the Eye. By Haynes Walton, Surgeon to the Central London Ophthalmic Hospital, and to St. Mary's Hospital. London: Churchill, 1861.

The Modus Propagandi of the Human Species Physiologically Explained. By John O'Reilly, M.D., &c. New York: Hall, Clayton, & Co., 1861.

Modus Operandi of Medicines. By the same author, 1861.

A Descriptive List of Microscopical Specimens. By Lionel S. Beale, M.B., F.R.S. London: Churchill, 1861.

On the Time and Manner of Closure of the Auriculo Ventricular-Valves. By George B. Halford, M.D., M.R.C.S. London: Churchill, 1861.

The Obliquity of the Fœtal Head in the Mechanism of Parturition. By J. Matthews Duncan, M.D., F.R.C.P.E. Edinburgh: Oliver & Boyd, 1861.

THE

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ORIGINAL COMMUNICATIONS.

- I.—*Cases illustrative of Diseases of the Ear.* By T. M'CALL ANDERSON, M.D., Physician to the Deaf and Dumb Institution, Physician to the Dispensary for Skin Diseases, &c., Glasgow.

No. I.

(Being the substance of a Paper read before the Medical Society, November 19, 1861.)

It is my intention to give jottings, from time to time, of some of the more important and interesting of the diseases of the ear, as I have noticed that, while cases of almost every kind of medical, surgical, and obstetrical practice appear daily in our medical journals, those illustrative of aural affections have been either altogether passed over, or only brought under the notice of the profession at rare intervals, and, even then, usually described in such loose and unsatisfactory terms, as to be almost invariably useless as guides in practice. I do not of course include in this category the many excellent cases cited in standard works on diseases of the ear, such as those of Toynbee and Wilde. I shall endeavour to make my observations as simple and practical as possible.

The first case is one of—

Bony Tumours springing from the Osseous External Meatuses.

W. W., Esq., aged about 50, consulted me on the 5th of July, 1861. Three weeks previously he had been seized with severe pain in the right ear, tinnitus, and deafness. Before consulting me he had been leeches and blistered by the family attendant, with relief to the pain.

On examination of the right ear, the tick of my watch was heard at the distance of $2\frac{1}{4}$ inches from the ear. The meatus auditorius externus was very roomy; and, springing from the walls of the inner third, close to the membrana tympani, five little

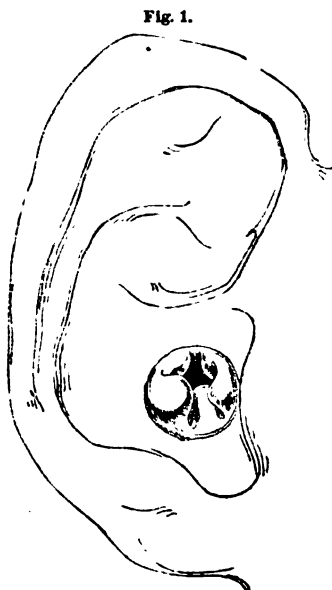


Fig. 1.

bony tumours were detected, such as are delineated in the woodcut—fig. 1—which gives a very accurate idea of the parts, with this exception, that the depth of the growths in the meatus has not been attempted to be shown.* These very nearly filled up the whole diameter of the canal, the intervals between them being choked up with epithelial debris, &c., the results of the inflammatory process. It appeared to me that the inflammation and deafness arose from the accumulation of epithelium and other matters between the tumours and between them and the drum, and the consequent pressure upon that organ. There was no doubt, therefore, in my mind, that, although it was impossible to see it, the membrane of the tympanum was inflamed, and the

mucous membrane of the cavity of the tympanum probably congested.

On examining the left ear, six little bony tumours were detected springing from the middle third of the meatus, which are well represented in fig. 2. The hearing on this side was perfect, and neither pain, uneasiness, nor tinnitus were complained of.

Both eustachian tubes were free, and the general health of the patient excellent. Neither gout nor rheumatism were in his family.

I directed treatment to the right ear principally; endeavoured to remove the extraneous matter from between and behind the exostoses, by the use of the syringe and tepid water; and to

* The drawings from which the woodcuts have been made, were taken from life by Mr. J. D. Bell, 12 Kelvingrove Street, Glasgow, who has frequently been engaged in similar drawings for me, which have been uniformly executed to my entire satisfaction.

subdue the remains of the inflammatory symptoms by means of repeated blisters behind the ear, an occasional sharp purge, and the eighth of a grain of the bichloride of mercury, twice daily after food.

On the 15th July the patient had only an occasional twinge of pain, and the tick of the watch was heard at the distance of a foot and a quarter. After this, a number of furunculi formed on different parts of the body, and one in the meatus; and a little later, after catching cold, the whole meatus became inflamed, with much swelling of the canal, but no discharge.

These symptoms having, under antiphlogistic treatment, subsided, the counter-irritation behind the ear, by means of blisters, was continued; the bony tumours were painted two or three times a week with tincture of iodine, and five grains of the iodide of potassium were administered thrice daily. This treatment was commenced on the 9th of August, and with the concurrence of Dr. Eadie, who saw the patient along with me.

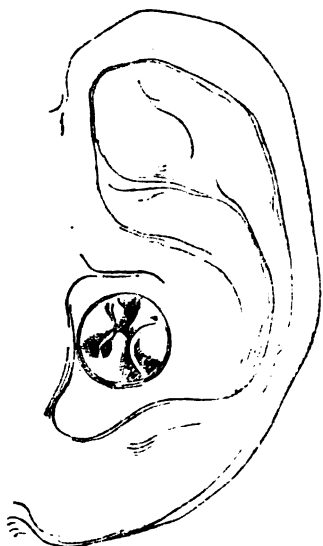
The local treatment required to be omitted twice, and antiphlogistics employed to remove attacks of inflammation of the meatus.

On the 10th September the blisters behind the ear were omitted, and the tincture of iodine painted over the mastoid process daily in their stead.

On the 19th September the exostoses in the left ear were painted for the first time with the tincture of iodine, and the same medicine applied over the mastoid process.

On the 25th the iodide of potassium was omitted, as it caused derangement of the stomach, and consequent loss of appetite and sleep, and great increase of the tinnitus. These symptoms at once subsided when it was stopped. It was necessary every now and then to drop almond oil into the meatuses for a day or two, and then to wash them out with tepid water, so as to soften and remove the masses of epithelium, &c., which collected on, between; and behind the exostoses; and, every second or third time that the tincture of iodine was applied, an epithelial cast of the exostoses separated and was removed.

Fig. 2



On the 23d October, two and a half months after the commencement of the iodine treatment, the hearing in the right ear was perfect; the tinnitus, which at first was so frightful that the patient said he thought he could not have lived had it continued, was completely gone, and the pain altogether removed. The tumours in the ear were slightly diminished in size, as the superior lozenge-shaped fissure was a good deal larger than formerly, and through it a small opaque portion of the drum was detected for the first time. The opacity of the drum proved the correctness of my previous diagnosis, of inflammation of that organ.

The complete removal of the deafness, I believe, however, to be more owing to the removal of the remains of the inflammation, the separation of the epithelial debris, &c., from between and behind the growths, and the thinning of the soft tissues covering them, than to the diminution in size of the exostoses themselves.

The tumours in the left ear were much in the same state as before. By this time the tincture of iodine had been applied twenty-five times to the exostoses in the right ear, and fourteen times to those in the left.

During the iodine treatment, the hearing in the right ear became several times so much impaired, that the tick of the watch was only heard when within half an inch from the ear, and each time the hearing became affected the tinnitus increased. These symptoms were relieved by the removal of epithelium, which had evidently become impacted between and behind the bones, and pressed upon the drum. At the patient's request I now wrote to Toynbee, who quite coincided in the treatment adopted, and, at his recommendation, I commenced the administration of small doses of quinine, in addition to the above treatment. By perseverance with the iodine I am in hopes that the deafness, tinnitus, and inflammation may be prevented from returning, and the tumours much diminished, if not altogether removed.

This class of aural affections has only been recognized within the last few years, and very few cases illustrative of the development of bony growths in the ear are on record. The only question, however, which I have time to discuss, is, whether iodine has the effect of diminishing the size of the exostoses. It appears to me, that in the case just related, the iodine has had the effect of slightly diminishing their size; and Dr. Thomas Keith of Edinburgh, who saw my patient also, wrote to me with regard to a similar case of his. He says:—"Since I last wrote you, I have had a note from a Maltese doctor about a patient I saw here last summer. His case was very similar to that of Mr. W., both meatus being quite blocked up by masses of dense bone, with a very thin cuticular covering. He now writes me that the tumours are disappearing in a wonderful way,

and that his patient hears so well, and has got so completely quit of the noises in his head, that he can with difficulty get him to persevere in the treatment. He had nothing but small doses of iodid. potass. for a month, and the application of the iodine to the tumours for a short time. The external irritation by means of the iodine had been kept up four or five months."

The cases related by Toynbee are unsatisfactory as regards this point. Of nine cases, quoted in his admirable treatise, there is only one in which the iodine was supposed by him to diminish the size of the tumour.

With regard to this case he says:—"Tincture of iodine was applied to the surface of the tumour, as also behind the ear, and four grains of iodide of potassium were given thrice daily, for between two and three months. Great relief was the result; the size of the tumour diminished; the power of hearing greatly increased; the tube of the ear lost its unnatural sensibility; and the unpleasant sensation of distension completely vanished."* Now, although undoubtedly the iodine in this case diminished the size of the swelling, I am by no means satisfied that the *bony* growth itself was materially reduced; for in the case which I have related, although the diminution in the size of the tumours has been slight, the hearing is now perfect; whereas, in Toynbee's case, the hearing power was only greatly increased, an improvement which can be quite accounted for by the removal of matter accumulated between the exostosis and the meatus, and by the thinning of the soft parts covering the bone; for, it must be observed, that a very narrow fissure indeed is sufficient for the transmission of the vibrations of sound, and the complete restoration of the hearing power.

From this case I pass to one which is allied to it, in as far as the disease consists principally of a deposit of matter not much different from bone, a case of—

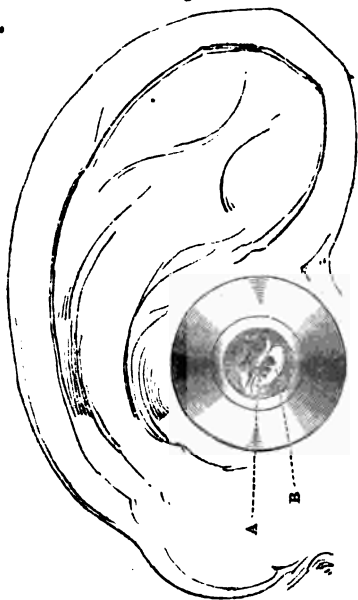
Calcareous Degeneration of the Membrana Tympani on one side, with loss of the Membrane on the other.

A. R., Esq., aged 58, consulted me on the 25th October, 1861. He stated that he had suppuration of the left ear in childhood, and that the hearing on that side had been considerably impaired ever since. About 18 months before I saw him, he noticed that he was becoming gradually deaf (the deafness being of course more appreciable in the right ear, which had previously been healthy), and this deafness, which increased rapidly, was accompanied by tinnitus.

* The Diseases of the Ear: their nature, diagnosis, and treatment. By Joseph Toynbee, F.R.S., p. 116.

On examination of the right ear, the tick of the watch, inaudible when applied to the temple, was heard when touching the ear; the meatus externus was dry and deficient in cerumen. The membrana tympani was to a great extent converted into calcareous matter, as may be seen from the accompanying woodcut—

Fig. 3.



A Handle of malleus. B Plate of calcareous matter, with spiculae running inwards towards the handle of the malleus.

fig. 3. It will be observed that at the edge of the drum, especially anteriorly and inferiorly, there is quite a solid rim of calcareous matter, giving off little spiculæ, which run in the course of the radiate fibres towards the handle of the malleus, which is quite distinctly seen. The intervals in the drum between the spiculæ, are quite transparent. The membrane was more concave than natural, and the eustachian tube impervious.

On examination of the left ear, the tick of the watch was heard when touching the ear, and also when applied to the temple. The left meatus was in the same state as the right; the membrane of the tympanum gone (the result, probably, of the inflammation in childhood) but the ossicles entire, though somewhat shrunken and rigid, and a

distinct thin fibrinous band was seen passing backwards and downwards from the point of the handle of the malleus. Old inflammatory deposits were detected in the cavity of the tympanum—fig. 4. The eustachian tube was impervious. The application of the artificial drum on this side did not improve the hearing in the least.

This gentleman had a severe illness some time ago, and although recovered from it when I saw him, he was not nearly so strong as formerly. He stated that deafness was in his family, as also rheumatism and rheumatic gout to some extent, but he was not himself subject either to gout or rheumatism. His mother died of apoplexy.

This case, which was also seen by my friend Dr. Scott Orr, his family physician, did not hold out much hopes of improvement. Glycerine was recommended to be dropped into each ear

occasionally, so as to remove the dryness of the meatuses. A leech was ordered to be applied to the orifice of each canal, and a mixture containing one grain of quinine and ten minims of dilute sulphuric acid to be taken thrice daily, in order to remove any congestion which might be present in the mucous membrane of the tympanic cavities. On the 2nd of November (the leeches having bled freely) there was no change in the right ear, but the tick of the watch was heard at the distance of one inch from the left. The patient stated that his friends remarked him to be more cheerful and hearing better.

On the 8th of November the hearing in the right ear remained as before, while the watch was heard at the distance of fully four inches from the left. The tinnitus was much moderated in both ears.

The quinine mixture, which was omitted a few days previous on account of headache, was to be taken only twice daily, and the mastoid processes blistered with glacial acetum cantharidis, the blistering fluid to be repeated as often as necessary, so as to keep the surfaces raw for some time.

In this case I fear that very little improvement can be expected to take place in the right ear, not on account of the degeneration of the drum, but because I believe this condition to be symptomatic of rigidity of the ossicles, and partial union of the base of the stapes to the margin of the fenestra ovalis. I am led to this opinion from the fact that the hearing is so much impaired, more so than can be accounted for by the state of the membrana tympani, and especially because the tick is inaudible when the watch is applied to the temple. This opinion is strengthened by the circumstance that, in dissections of the ear, calcareous deposit in the drum has been frequently noticed to be associated with a similar degeneration of the deeper seated parts.

It is important, however, to continue the treatment in this case, for, by improving the tone of the system, we are enabled

Fig. 4.



A Head, B handle of malleus. C Fibrinous band passing backwards from handle of malleus. x Deposits in the cavity of the tympanum.

to diminish the tendency to congestion of the ear, leading to a further degeneration of tissue and a further impairment of hearing.

On the left side, the marked improvement in hearing is due to the removal of the congestion by the leeches, &c., and to the lubrication of the parts by means of the glycerine. The total failure of the artificial drum I believe to be owing to the old inflammatory deposits in the cavity of the tympanum, the consequent rigidity of the ossicles and obstruction of the eustachian tube, and probably also to the commencement of a similar degeneration of tissues, as in the right ear.

This case leads me to the consideration of the conditions favourable to the use of an artificial drum. These have not been referred to at any length in works on aural surgery, and I must therefore be excused for giving my own conclusions; founded upon the application of the drum in a very considerable number of cases, the pathological appearances in which were very various, and very carefully noted. Of course I take for granted in these remarks that the internal ear and auditory nerve are healthy:—

1st. With regard to the membrana tympani itself. It is of course necessary that there should be an orifice in the drum; and, if the latter be very much thickened, it is desirable that the orifice be large, as the vibrations of sound are only to a limited extent transmitted through a membrane in such a condition. On the other hand, I believe it to be very desirable that the drum be not entirely gone, but that at least a fringe of it should remain; for if the whole of it is gone the artificial drum is apt, unless the operator be very skilful, to be pushed further inwards than the seat of the previous membrane, and the cavity of the tympanum is thus liable to be plugged up—a condition which most perceptibly diminishes, instead of increasing the power of hearing. If a fringe of it remains, however, the artificial drum, on being gently introduced, comes against these fragments, and we are thus warned that it is far enough inserted.

2nd. With regard to the state of the eustachian tube. It is very desirable that it should not be obstructed, for its patency is a guarantee that air shall constantly have access to the cavity of the tympanum—a condition of great importance for the transmission of the vibrations of sound to the internal ear.

It may be said, no doubt, that air can enter the tympanum through the orifice in the drum; but if the artificial drum be applied so as to fill up the orifice completely, the air can no longer of course freely enter the cavity of the tympanum. In cases, therefore, where the eustachian tube is impervious, I believe it to be of moment to introduce an artificial drum of

such a size, and to place it in such a position that it will not altogether prevent the passage of air into the cavity of the tympanum.

For another reason also is it favourable to the use of an artificial drum that the eustachian tube should be pervious; for, given two cases, in one of which the eustachian tube is pervious, in the other not, we are led to suspect that the primary inflammation in the latter instance has been more severe, and done more damage to the structures of the ear generally, than in the former.

3rd. With regard to the state of the ossicles. It is to be desired—though this does not seem to be so necessary to the success of an artificial drum—that the ossicles be entire; and this partly for the reason before adduced, that the less severe the primary inflammation the less likely are the ossicles to be destroyed, and *vice versa*. Being present also, they should materially assist in the transmission of the vibrations of sound to the internal ear; but it appears to me, that the reason why their absence is not of such moment as at first sight might seem is, that, when present, their action is very usually impaired by rigidity, owing to the presence of inflammatory deposits upon them, and bands of false membrane passing between them.

4th. With regard to the state of the cavity of the tympanum. It is of great importance that it should be free, and not filled up either partially or entirely by the results of the previous inflammation. You will probably have noticed, in many cases, that the cavity of the tympanum is very much filled up with red fleshy granulations, and that the introduction of an artificial drum produces only slight improvement in the hearing power, so slight that I think we are not usually justified in recommending the patient to trouble himself with it. If the inflammation of the tympanum has completely passed away many years before the patient comes under observation, I have noticed that there is often no longer any trace of fleshy masses. These having shrunk away, the cavity is apt to be filled up with thoroughly organized cellular tissue. This condition is also very unfavourable to the use of an artificial drum.

On the other hand, although the cavity of the tympanum is quite empty, its mucous membrane is often much thickened and congested, this being owing in all probability to its having lost its tone from the previous inflammation, and to its being affected, in the absence of the drum, by the access of cold air from without. This condition also tends to diminish the beneficial effects of an artificial membrane, and must therefore be attended to.

When the mucous membrane is pouring out a discharge of muco-purulent or purulent matter, which has often a very nauseous

odour, it is useful to keep the cavity quite clean by the repeated use of tepid water, and to moderate or stop the discharge and diminish the congestion by astringent injections, &c.

Sometimes the discharge is of the nature of lymph, and a layer of it comes to occupy the site of the lost membrane. This I have repeatedly noticed, and one is very apt to mistake it for an opaque drum perforated by two or three holes, for it rarely plugs up the whole orifice. On using the syringe, not only does this layer of lymph disappear, but—and this is very curious, as illustrating how nature provides for such deficiencies—the hearing power is actually diminished, the lymph having apparently acted the part of an artificial drum.

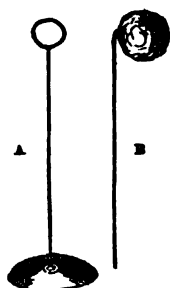
The cases, then, which appear to me to be most favourable to the use of the artificial drum are those in which the following is the state of parts :—the internal ear and auditory nerve healthy ; the ossicles present and not rendered rigid by deposits of inflammatory matter, or by ossification of the articulations ; the eustachian tube free ; the cavity of the tympanum empty and roomy, its mucous membrane not much thickened and congested ; the absence of much discharge, and the presence of a small fringe of the membrane of the tympanum. Very few cases will occur in which all of these conditions are met with, but the more of them present the more benefit is likely to accrue.

With regard to the *rationale* of the action of the artificial drum, I am quite inclined to coincide with a recent writer in the *British and Foreign Medico-Chirurgical Review*. "Given, a drum with a hole in it to restore its resonance. Answer—Stop up the hole, or put a new skin on."

As to the form of drum to be used. Most are in the habit of

employing either a moistened pellet of cotton, according to the original idea of Yearsley, to whom we are indebted for the discovery—a discovery which I have no hesitation in characterizing as one of the most important in the medical science of the present age—or the elegant little drum which Toynbee recommends—see fig. 5, A. I am in the habit of using a little contrivance which combines, I think, the excellencies of each method without their defects. I keep a small stock of silver wire by me, and, when I have need, I cut off a piece about an inch long, bend it at one end into the shape of a small hook, insert a small pellet of cotton within the hook, and fix it by squeezing the two sides of the hook together—see fig. 5, B. This little combination enables the patient to introduce and remove the cotton with ease, and has thus an advantage over

Fig. 5.



the simple pellet of Yearsley, while it has the advantage over Toynbee's artificial drum of being, I think, less irritating, of costing nothing, of being capable of being made at any moment by the patient, and of thus obviating the necessity of re-introducing a drum which has been soaked in purulent matter.

I have selected two cases in illustration of the beneficial effects of an artificial drum, with which I shall bring to a close these already too lengthened remarks.

(1.) Mrs. F., aged about 30, came to me on the 21st of August, 1861. She complained of purulent discharge from the right ear of ten months' duration, with slight occasional pain and tinnitus, the latter especially on agitation. The discharge was not constant, but came and went. It was slight when I first saw her, and she said that sometimes an exfoliation from the meatus of a "dry scurf" took the place of the discharge. An enlarged gland was detected on the right mastoid process. The tick of the watch was heard at the distance of six inches from the ear. The meatus was pretty natural, but the membrana tympani was much thickened, opaque, and white. The handle of the malleus was seen as a thick, white, curved line, not very prominent. A small round orifice, about a line and a half in diameter, with smooth thick edges, was detected immediately behind the handle of the malleus. The eustachian tube was pervious, as air whistled through the orifice in the membrane on forcible expiration, the mouth and nose being closed.

The left ear was healthy, and the general health pretty good, except that the patient did not go out of doors much, and she was much exposed to draughts.

An eighth of a grain of the bichloride of mercury was ordered twice daily, blisters over the mastoid process, and an injection of the chloride of zinc (gr. ii. to the ʒi. of water) used twice daily.

On the 7th September the discharge was all but gone, and the tick of the watch heard at the distance of seven inches. A little ball of cotton soaked in almond oil was introduced gently over the orifice, and, when properly adjusted, the tick was heard at the distance of two feet.

Mrs. F. received several lessons upon the application of the cotton, and was then dismissed.

(2.) A medical man in town consulted me a few months ago concerning deafness in the right ear. At the age of fifteen months he had a severe attack of scarlet fever, with bad throat affection. When recovering from this attack he caught cold, which was followed by a relapse of the sore throat, and by supuration of the right ear. The discharge had continued on and off ever since, being sometimes only very slight; at other times,

on catching cold, pretty profuse. On examination of the ear the discharge was slight, the meatus natural, the membrana tympani gone, with the exception of a narrow, thickened, red, fleshy-looking fringe posteriorly. No trace of ossicles to be seen. The cavity of the tympanum was roomy; its mucous membrane smooth, red, and lubricated, with discharge; and the eustachian tube quite free, so that when the ear was syringed, water ran down the tube into the throat and nostril.

The tick of the watch was heard when applied to the temple and when touching the ear; and the voice could only be heard on this side when loud and close to the ear.

The artificial drum was introduced, and, when properly adjusted, the tick of the watch was audible at a distance of seven inches, and the patient could hear ordinary conversation with perfect ease when at the very opposite end from myself of a pretty large room. Whenever the drum was removed the deafness returned.

II.—*Successful Case of Tracheotomy in Croup and other Surgical Cases in Private and Hospital Practice.* By GEORGE BUCHANAN, A.M., M.D., Surgeon to the Glasgow Royal Infirmary, &c.

AGNES M'L., aged 6 years, was seized on the 6th of March, 1861, with symptoms of croup. She had a hoarse barking cough, difficulty of breathing, and hot skin. Dr. Samuel Clark, the family medical attendant, on the same day took about six ounces of blood from the arm, with some relief to the oppression, and ordered two grains of calomel every two hours. In the evening she was a little better; but the hoarse cough was still more severe next morning, and the difficulty in breathing greater. A fly blister was applied to each side of the neck. Next day she seemed a little easier in the morning, but got worse in the evening; and at half-past seven the breathing became very laboured, and she seemed almost choked. The above account I got from her parents and Dr. Clark; the following are the notes of my connection with the case from that time:—

On the 8th of March, at 8 p.m., I was hurriedly summoned to visit a child who was said to be dying of suffocation from croup. I seized my pocket-case and an elastic catheter, and accompanied the messenger to the house which was only a few doors off. On arrival I found Dr. Drummond in attendance, who informed me that he had been sent for a short time before, and had just arrived in time to see the child on the point of dying from asphyxia. I found the pulse almost imperceptible; the breathing stopped

except once an ineffectual attempt at respiration ; the skin cold ; the lips and face blue ; the muscles relaxed ; and every appearance of immediate death. It seemed too late to hope for recovery. Rapidly we told the parents, who supposed they were looking on a lifeless child, the remote chance there was in tracheotomy ; and as the case was hopeless at any rate, they consented to anything we might think reasonable to undertake.

I placed the child upon the bed, with the shoulders supported by a pillow, and made an incision over the trachea an inch and a half in length. In the cellular tissue a vein was distinctly visible, which was held aside while I laid bare the rings of the trachea by a few touches of the scalpel. I now plunged the knife into the trachea and extended the incision half an inch, then introducing the points of a pair of forceps, withdrew the knife. By separating the points of the forceps, the aperture could easily be widened. The opening of the trachea was immediately followed by a rush of air, and presently a prolonged fit of coughing took place, which expelled from the wound, and also partly through the glottis, a quantity of tough mucus and shreds of what seemed false membrane. Respiration was exceedingly feeble at first, but gradually the breast began to heave ; the pulse became perceptible, and the lividity of the lips passed off. Several fits of coughing took place, each of which expelled some of the same substance which came away at first, and in a short time the air passage seemed to have become clear enough for the air to pass freely to the lungs, when the wound was held open by the forceps. I tried to introduce a piece of catheter to keep the passage free ; but each attempt was accompanied by such irritation that I gave up all intention of persevering. Besides, even if a tube or piece of catheter could have been introduced, it could not have been fixed in its place, as the raw surfaces of the blisters would not have borne the pressure of the tapes necessary to retain it. Fortunately there was no hæmorrhage, so there was no annoyance from blood trickling from the wound into the trachea. After holding the lips of the wound apart for nearly an hour with the forceps, I found that the trachea had become comparatively cleared, and that respiration, though laboured, was going on regularly ; and the heat of the body was nearly restored. I also found that I could withdraw the forceps for a short time ; and that when I did so, respiration went on, though feebly, partly through the glottis, and partly through the wound in the trachea which was well exposed by the free outer wound, and remained open. At intervals the wound got choked up, when I had to introduce the forceps and re-open it ; but in a short time this was not required. The atmosphere of the room was rendered suitable to enter the lungs, by having diffused in it the steam from a

boiling kettle, as also from pans of boiling water placed beside the bed within the curtains. I remained all night in the house beside the child, and once or twice sponged or cleared away with a probe some bronchial secretion which was blocking up the wound. Respiration went on comparatively tranquilly all night, and the child slept at intervals.

Next day she continued much in the same state, breathing with some embarrassment partly through the mouth, and partly through the aperture. Occasionally the wound was kept clear by sponging away some tough bronchial secretion which came away by the effort of coughing. She took a little beef-tea and wine negus which was swallowed without much difficulty. The temperature of the room was carefully attended to, and steam was kept constantly diffused by the means above mentioned. I need not continue daily formal reports, but will add that the little patient continued to improve till the sixth day, when I found that respiration was going on so freely through the larynx that the wound could be shut up without annoyance. A piece of carded cotton was placed in the wound and retained by a loose bandage round the neck. On the fourteenth day the wound was so much decreased in size by the process of granulation and contraction that I could easily bring the edges together with a strip of adhesive plaster. In three weeks cicatrization of the wound was complete, respiration was free; the voice had returned, though still feeble; and the child was able to run about the house and play with the other children; and I gave up regular attendance at the end of March. Her health, however, was much weakened, and a tendency to stoop, which she had before her present illness, was increased. About a month after the operation it was observed that occasionally she had a slight squint in one eye; but this was only noticeable at intervals.

Dr. Clark was visiting the other children in the family on the 16th of April, and being informed of the tendency to squinting just mentioned, ordered a purgative of calomel and scammony, which was given immediately. Vomiting came on soon after this, and continued without intermission for several hours. In the evening I was sent for to see the child, whom I found quite exhausted from the severity of the sickness and retching. I ordered hot fomentations to the belly, and warm brandy and water to be given frequently in small quantities. The vomiting soon ceased, and the patient continued quiet but weak all night. Next morning she seemed somewhat better; but about 2 p.m. she was seized with a convulsion fit, in which she died. Dr. Clark was in the house at the time, and saw her in the convulsions. He sent me the following note regarding the manner of her death:—

"My dear Sir—I am exceedingly sorry to inform you that your late very interesting patient, without any manifest cause, took a convulsion fit between one and two o'clock, and died about a quarter past two. I saw her during the fit; the respiration was quite free, though her mother said she had been complaining of her throat in the forenoon. I looked into it and found the velum and tonsils quite natural; and to convince her mother that it was not much affected, I gave her a spoonful of water which she swallowed without difficulty; but in a very short time afterwards she went into the last agonies with perfectly free breathing. She walked rather crooked for two days past, and did not like her neck and spine to be stretched out; but we had no anticipation of any such event being so near.—Yours truly,

SAMUEL CLARK."

"Dr. George Buchanan."

I got liberty to make a post-mortem examination of the neck which I did with the assistance of Dr. Clark. The wound in the integument was perfectly healed, and the cicatrix was adhering to the trachea. On removing the larynx and trachea, the mucous membrane lining both organs was found perfectly free from disease. The rima glottidis was quite normal, both in size and colour. The mucous lining of the trachea was quite healthy, and the line of the incision was plainly seen, but perfectly united and smooth. It was quite obvious that the cause of death was not to be found in the trachea, which had regained a perfectly healthy condition since the operation.

I have only to remark in conclusion, that while the question of tracheotomy in croup is a wide one, and admitting of powerful arguments on both sides, the success of the present case was such as would lead me to recommend and perform it without hesitation in similar circumstances. There may be doubts as to the propriety of cutting into the trachea in a certain stage of croup as a means of preventing the suffocation which will probably ensue, if the case go on unchecked; but that is a totally different case from the present where the suffocation had already occurred, and would inevitably have caused death in a few minutes more, if the air had not been admitted by tracheotomy. I believe, that in a case of immediate danger of death by suffocation, we are not only warranted, but called on, to give the patient the chance of life by performing tracheotomy.

Lithotomy—Two Triangular Stones.

The following case of stone in the bladder is interesting from the remarkable shape of the calculi.

Samuel E., aged 11, has been troubled with symptoms of

stone at intervals for nine years. About three years ago the symptoms were very acute, and he was sounded, but at that time no stone was detected. Shortly after that the severity of the suffering passed away, and he continued in comparative comfort till the end of October, 1861, when after a long walk the symptoms were much aggravated, and the pain became excessive. For a fortnight he continued to suffer greatly, unable to sleep, or to continue in one posture many minutes at a time. At this time I was asked to see him by Mr. Christie, surgeon, Bridgeton, whose patient he was; and putting him under chloroform, I at once detected the presence of a calculus.

On the 16th of November, I operated, using the rectangular staff, and without difficulty removed two stones of a very remarkable shape. Both together weighed an ounce and a quarter; the larger being three quarters, the smaller half an ounce.



They are precisely similar in form, that of an equilateral triangle with the angles very slightly rounded off. The sides of the larger measure one inch and two fifths, its thickness is three quarters of an inch; the sides of the smaller measure one inch and a half, its thickness is half an inch. The chemical composition of the stones, as analyzed by Dr. Penny, is as follows:—

Phosphates,	.	.	.	55	per cent.
Carbonate of lime,	.	.	.	7	"
Lithic acid,	.	.	.	28	"
Water,	.	.	.	10	"
				100	

The boy recovered without a single unpleasant symptom, being perfectly freed from his former agony from the date of the operation.

The annexed woodcuts give a faithful representation of the calculi.

Removal of a Piece of Catheter from the Female Bladder.
(From Notes by Dr. Agnew.)

Mrs. A., aged 36, was admitted to the Royal Infirmary on the 18th of October, 1861. She gave the following account of her case:—Nine years ago, when three months advanced in pregnancy, she experienced much difficulty in voiding urine, which required repeated catheterism. In a subsequent pregnancy six years ago the same thing occurred. At present she is again pregnant, and about a fortnight ago the inability to void urine occurred as before. Having experienced relief from the use of the catheter formerly, she of her own accord endeavoured to relieve herself by using an instrument something like a catheter, made for her by her husband who is a worker in gutta percha. On passing it into the bladder, either through awkwardness in manipulation, or from the insufficient nature of the instrument, a piece about three inches long was broken off and remained in the bladder. Several attempts were made by a surgeon to detect and remove it, but without effect, and she continued to suffer great pain and inconvenience from its presence till the present time.

On admission she complained of great pain in the lower part of the belly, and any attempt at voiding urine was attended with increased suffering. She was weak and anxious-looking. I saw her on the morning of the 19th, and proceeded to extract the foreign body. She was put under the influence of chloroform, and placed in the lithotomy position. I found the urethra so much dilated by the previous attempts that I could introduce the point of my little finger, so that a pair of forceps were without difficulty slipped into the bladder. Although I could easily grasp the foreign body, its peculiar shape prevented me from laying hold of it in a position favourable for extraction, and it was only



after repeated attempts that I at last succeeded. This was accomplished by seizing it in the middle, and pulling it forwards against and across the urethra. I held it in this position by introducing the left fore-finger into the vagina, and pressing the foreign body against the pubis. I now opened the forceps and gradually shifted them to one end of the tube and then easily turned the free end into the cavity of the bladder. The body, being thus grasped by one end in a longitudinal direction, was slowly pulled through

the urethra. It was found quite coated with calculous matter, and as some pieces had been scraped off by the forceps, the bladder was washed out with a stream of tepid water. The patient was ordered bland fluids to drink, and she soon recovered from her distress. She could retain her urine on the second day after the tube was extracted. The tube was three inches long, and was of the shape and dimensions here figured.

Dislocation into Axilla, with Compound Fracture of Humerus—Reduction—Recovery.—(From Notes by Mr. R. Leishman Allan.)

Michael M., aged 60, was admitted to the Royal Infirmary on the 17th May. He stated that this morning when driving a cart, the horse became unmanageable and ran away, in consequence of which he was thrown to the ground and injured seriously.

On examination he is found to have sustained a compound fracture of the left humerus in its middle third. The external wounds, two in number, are very small, and are situated the one posteriorly and the other anteriorly. The fracture itself, though not comminuted, is of an unfavourable description. The head of the humerus is found to be displaced and resting in the axilla; patient states that attempts were made previous to his admission to reduce the dislocation.

18th—The opinion of a consultation was that an attempt should be made to reduce the dislocation, and that the arm should have a chance, though grave doubts were entertained of its ultimate recovery without amputation. The case was managed in the following manner. I first applied a straight splint to the arm, bandaging it from the hand to near the shoulder, and fixed a skein of worsted to the arm as near the shoulder as I could place it. Chloroform having been administered, I placed my left heel in the axilla, and after considerable exertion, had the satisfaction of feeling the head of the bone leaving its abnormal situation and slipping into the glenoid cavity. The bandages were then removed, and the wounds over the seat of fracture dressed with dry lint, and the arm put up with a bent splint.

25th—Complains of no pain and is in good health and spirits. The wounds having given little annoyance, the bandages have not yet been removed.

29th—As patient complains of a little uneasiness at seat of fracture, the splint and bandages were taken off, when it was found that the wounds had both healed kindly, and the case was thus converted into a simple fracture of the humerus. From this time the patient progressed favourably, and he was dismissed well at the end of six weeks.

Double Amputation for Injury to both Arms—Recovery.
(From Notes by Dr. F. Henderson.)

Amputation of both limbs at the same time is a comparatively rare occurrence; the following is the only case which has occurred for some years in the hospital, and one of the few which has terminated favourably:—

John O., aged 43, was admitted to ward 10 of the Glasgow Royal Infirmary on the 11th of January, 1861. He stated that about eleven o'clock the same day, the beam of a crane fell on his arms, which were stretched out before him, causing the present injuries. The left arm was found to have sustained a compound comminuted fracture a little below the elbow, the fractured ends of the bones were protruding, and the muscles and soft parts were completely destroyed. The right hand was smashed up to the wrist joint, the bones fractured, and the skin and muscles reduced to a confused pulpy mass. There was a scalp wound on the back of the head about three inches long, the bone uninjured. The right knee was slightly bruised. The man would not consent to immediate amputation, but at 8 p.m. a consultation was called when he agreed to submit to what was necessary. I removed the right arm immediately above the wrist, and the left just above the elbow. Both stumps did remarkably well; but the recovery after such a combination of injuries was necessarily tedious. I saw him in October last, and he had great freedom of motion and power in both arms. He wears a hook on the right stump; the other is quite sound, but he has not yet got any apparatus adjusted to it.

III.—*Illustrations of Puerperal Diseases*. By R. UVEDALE WEST, M.D., Fellow of the Royal College of Surgeons, Edinburgh; Vice-President of the Obstetrical Society of London, &c., &c.

(Continued from page 282.)

OF PUERPERAL FEVER REGARDED AS AN EPIDEMIC.

In the following series of cases, although the notion of infection conveyed by myself and communicated by manipulation would seem to be invalidated by the fact that so many intermediate cases where there was much even *meddlesome* manipulation, escaped altogether, I feel, nevertheless, that no other hypothesis will explain the facts, unless indeed we may look on the whole series as an epidemic—a view which would certainly

seem to be corroborated by the occurrence of well marked diphtheria as a symptom in two of the cases (Cases XLVI. and XLVII.), that disease being very rife at the time throughout the district in which the puerperal fevers occurred. At any rate, the epidemic of diphtheria imported itself into my puerperal fever cases;—a proof of the force of epidemics, if of nothing else.

On the 4th of March, 1859, I was hastily summoned to visit a woman in the country who had three days previously been delivered by a midwife after an average easy labour—fifth pregnancy. I found her suffering from acute peritonitis, with a peculiar swollen condition of the abdomen, which I judged, from what I could learn of the patient's previous history, was caused by the presence of an ovarian tumour. The woman was very bad* and died three days after, March 7th. I found, on a post-mortem examination which I made the following morning, a large unilocular cyst of the left ovary filled with the usual fluid and occupying the abdomen, so as to push up the small intestines into the left hypochondriac region. The peritoneal covering of the cyst was black and gangrenous, as was that of the small intestines. The colon was much distended with flatus. It was, therefore, a case of acute peritonitis, involving chiefly the peritoneal covering of an old standing ovarian cyst, and terminating rapidly in gangrene. My attendance on this case, and especially perhaps the post-mortem examination of the subject of it, were followed in my midwifery practice by such an unusual proportion of febrile diseases of the puerperal state, caused either by direct contagion or by some epidemic influence, that I think it may be instructive if I introduce in this place some account of the whole of the cases of midwifery attended by me during the months of March, April, May, and June; with which last month the epidemic, if epidemic it was, ceased. I may premise that having felt a wholesome dread of the possible consequences to my lying-in patients of my having conducted this unlucky autopsy, I took every possible precaution, by way of disinfection, such as many ablutions, entire change of wearing apparel, &c., to obviate what I was afraid would take place. From the following narrative it will be seen that on a gross total of forty-eight midwifery cases attended between the 8th of March, the day of the post-mortem examination, and the end of June, there were ten cases of puerperal disease sufficiently grave to excite considerable apprehension for the result, although they all recovered. This was surely a very undue proportion. Although some of them, as did the case from which they all appeared to originate, exhibited inflam-

* A full report of this case stands recorded in the 1st volume of the "Obstetrical Transactions," page 187; it is therefore unnecessary to repeat those minute details here.

matory symptoms, they were all, equally like that case, more or less asthenic in type, and were benefited by stimulant or tonic treatment. A brief notice of the circumstances attending all the labours, as well of those women in the series who were not attacked with any puerperal febrile disease as of those who were, will show how very little influence such circumstances have in the production of those diseases, even during an epidemic of puerperal fever. Madame Boivin says, "Que l'accouchement de quelque genre qu'il ait été, lorsqu'il s'est opéré par les voies naturelles, n'influe presque en rien sur la nature des couches. Nous avons vu un grand nombre de femmes qui étaient accouchées naturellement et très promptement, être frappées de maladies fort graves et même mortelles; tandis que d'autres, qui étaient accouchées, soit au moyen de la version de l'enfant par les pieds, soit par l'application du forceps, même celles qui n'avaient pu accoucher qu'après la perforation du crâne de leur enfant, n'avaient pas ressenti le plus léger symptôme de maladie."* That all this is absolutely true, I am quite convinced from my whole experience. In the course of the well-known discussion on Puerperal Fever in the French Academy of Medicine in the spring of 1859, M. Serres (d'Alais) is reported to have said, "Il faut tenir compte d'abord du traumatisme qui résulte du passage de la tête à travers les parties génitales; puis de l'hémorrhagie, qui prédispose si puissamment à l'ébralement nerveux; enfin de la plaie placentaire," &c. &c. To this, which expresses accurately and concisely the prevailing prejudice, M. Depaul, very properly and very conclusively, replied "Les faits invoqués dans la discussion ont montré que le traumatisme ne jouait qu'un rôle très secondaire dans le développement des accidents. Les femmes les plus dilacérées pendant le travail, même en temps d'épidémie, et placées au foyer de l'épidémie, ne sont pas atteintes par la fièvre puerpérale." It is only fair to M. Serres, however, to point out and acknowledge, that in the following series, the *hæmorrhagic* cases were followed by puerperal symptoms in a proportion greater than would be explicable by coincidence merely. I think Depaul is quite right about the innocuity of *traumatism*.

On the 9th March, the day after the post-mortem examination, I attended a case of midwifery. It was an average natural labour, multiparous; but being rather lingering, I gave a dose of ergot of rye. The after-pains were severe, but otherwise the woman recovered without any unpleasant symptoms.

On the 11th March I attended another case. Multiparous—quick, easy, natural labour—good recovery.

* *Mémorial de l'Art des Accouchements*, Tome i. p. 442.

On the 13th I attended two cases, of both of which I must report as above.

On the 14th I attended two cases, both of which made a good recovery, although of the latter of them I had to report "after-pains severe from retention of a portion of the membranes." This was next door to the house in which I had made the post-mortem examination.

A primiparous woman, delivered with the vectis after a hard labour on the 15th, had abscess of breast in fourth week, from mismanaged lactation. There was little or no constitutional disturbance. So far I began to hope that there would prove to be no contagion; but it is bad taste to shout before we are out of the wood.

CASE XLIV.—METRITIS, PERITONEAL FEVER.

Mrs. F. of W.—the village where I had had the post-mortem examination, was confined early in the morning of the 16th of March. Multiparous. A languid labour with vertex presentation. I delivered her by turning, after waiting several hours for pains, which ergot of rye failed to arouse. The removal of the placenta was followed by great hæmorrhage with faintings and vomitings.

March 17th—Has had a great deal of pain with considerable hæmorrhage. Had a long rigor last night. Pulse now, 11 a.m., 125, very weak and thready, perhaps hæmorrhagic. There is headache; countenance pale, not anxious; tongue quite clean; there have been frequent vomitings; milk coming abundantly; perhaps the rigor was only a "milk rigor," but I do not like the symptoms altogether; abdomen soft but sore, body of uterus easily felt, and very tender to touch. To foment and take:—

R. Magnesiae sulph.	℥vj.
Tinct. opii	℥ij.
Spir. æth. nitrici	℥ss.
Tinct. card. c.	℥ss.
Aquæ	ad ℥vj. M.
Sumat coch. ij. quartis horis.	

18th—Better in every respect; pulse 96, fuller; plenty of milk; the bowels have acted, and she has expelled many coagula; she has no appetite. To take a few grain-doses of quinine. She had no further unpleasant symptoms.

CASE XLV.—PERITONEAL FEVER.

Mrs. D. of M., the wife of a farmer, multiparous, was confined March 18th. She had a remarkably easy and quick labour, but soon after the removal of the placenta she had profuse hæmorrhage

attended with frequent faintings, blindness, and deafness. The hæmorrhage was checked by the usual means.

March 19th—Had a rigor at noon. At 3 p.m. pulse 120, full, reactive; restlessness; headache; nausea; no pain in abdomen; milk coming. Ordered to take Mist. sennæ comp. until the bowels acted.

20th—Pulse 120; bowels not moved yet; appetite bad; complains of thirst; is hot and feverish. To take some more "black dose."

22nd—Feverish; has had another rigor with a good deal of headache; head better this morning; countenance good; no appetite; tongue white; milk in breasts; lochia scanty and watery; abdomen soft and free from pain, but there is some finching when pressure is made on *fundus uteri*; bowels moved once on the 20th, but not since. Pulse 120, rather smaller. To take Mist. puerp. and foment abdomen.

23rd—Better; no headache; less fever; pulse 104. Bowels well moved this morning; appetite coming; lochia and milk as yesterday. Continue.

25th—Feels quite well; pulse 96; a good deal purged yesterday. Some short time after her month was up, she had a slight attack of pneumonia, after which she got quite well.

On the 26th of March I attended an easy multiparous case of midwifery, the woman "getting away" without any untoward symptoms.

On the 27th I attended a multiparous case, which was so tardy that I delivered by turning. The woman recovered without any unfavourable symptoms.

On the 29th I attended a multiparous case, which was very easy and quick, but was followed by severe after-pains with considerable hæmorrhage. This woman had troublesome peritoneal fever, terminating in purulent elimination, so that her case may be found already reported in detail in this paper (Case XIX.) in the pyogenic group (p. 146).

On the 31st of March I attended an easy, natural, multiparous labour; the subject of it recovering without the development of any unfavourable symptoms.

On the 2nd of April I attended a languid labour, in which I gave the ergot of rye. No bad symptoms.

CASE XLVI.—PUERPERAL FEVER, DIPHTHERIA, ETC. ETC.

Mrs. St. P. R., of H., the wife of a farmer, was confined on the 3rd of April. She had been out of health some time, suffering from facial neuralgia and general debility. After a very tedious hard labour, ergot given, and ultimate difficult delivery with the vectis. The child was dead, and certain signs of incipient

putridity showed that it had died some days before its birth. I stayed with her two hours, and left her quite comfortable, pulse full, 70. After which I have the following notes of her case; it was the most alarming and troublesome one of the series.

April 4th, 10 a.m.—The after-pains have been severe all night, and she has had no sleep; complains of pain and soreness all over, and of much headache; she is lying on her left side, with her knees drawn up, and she cannot turn over; skin warm and comfortable; pulse pleasant, but 126; no appetite; lochia sufficient; countenance good; no hurry of respiration. There has been no rigor.

R. Calomel. et opij, āā gr. iij, statim sumend.;
et, post semi-horam, Ol. Ricini, ℥j.

The abdomen to be fomented.

5th—Position as yesterday; a bad night from pain; easier this morning; pulse still 126, soft and pleasant; abdomen full and soft, no tenderness on pressure; lochia sufficient; no milk as yet; appetite bad; complains still of headache, chiefly over one eye—it is the old neuralgia; bowels well moved yesterday; skin open and pleasant; no rigor; respiration good.

R. Magnesiae sulph.	℥j.
Pulv. ipecac. comp.	℥ij.
Sp. æth. nitrici	℥ss.
Tinct. card. comp.	℥ss.
Aquæ m. pip.	ad Oss.
Sumat coch. ij.	quartis horis.

Sumat horâ somni anod. (Liq. morph. acet. ℥j.) horâ somni.

6th—Much the same as yesterday; pulse soft, and with moderate volume, 128; abdomen very full and very tympanitic, soft and bearing pressure all over, except in right groin where there is tenderness; lies on her back with knees down; skin cool and open; countenance good and pleasant; no headache; tongue clean; bowels have again been well relieved; some dysuria last night; complains of vulva being swollen; warm water to vulva; turpentine stupes to abdomen.

Rep. Mist. cum spir. ammon. aromat. ℥ss, vice magnesiae sulph.

R. Camphoræ	℥ss.
Solve in chloroform	℥j.
Adde. ol. lini	℥ij.

Ut ft. linimentum, diligenter abdomini et inguinî infricandum.
Sumat horâ somni haustum anod. ut antea.

Same day, 8 p.m.—Great alarm, sudden and hasty message.

Found her in a hot, feverish state, with diaphoresis. Had been vomiting; and the pain in the abdomen, which had been gradually increasing since I left in the morning, had been much relieved ten minutes before my arrival by a copious evacuation from the bladder, and the expulsion of several coagula from the uterus. Abdomen soft and bearing pressure well, though tender with some hardness on left side; pulse 140; head very free from pain and very clear; countenance good; she is very smiling and inclined for conversation. It is probably a condition of reaction, and not unfavourable. Pergat, and let her take the draught as ordered.

7th, 10 a.m.—Has had a good night; feels some pain this morning; abdomen amazingly swollen and tympanitic, but bears pressure well; no soreness now in groins; pulse 104, very soft and distinct; warm perspiration all over; some slight appearance of milk in breasts; bowels and bladder relieved twice last night; no headache. Gave a turpentine enema, which caused the expulsion of a great quantity of flatus; and the bladder being again relieved, she felt much better when I left her at 11; complaining, however, of thirst and dryness of mouth.

R. Potassæ bicarb.	ʒij.
Tinct. card. comp.	ʒss.
Aquæ menth. pip.	ad ʒvj.
R. Acid. tartaric.	ʒiss.
Spir. æth. nitrici	ʒss.
Aquæ	ad ʒvj. M.

Sumat āā coch. ij. in actu efferv., quartis horis.

Same day, 8 p.m.—Has felt better all day; no milk; skin hot, with diaphoresis; not much pain; abdomen not quite so much distended; has been slightly delirious all day; pulse 134; is very restless and depressed.

Habeat statim enema terebinthinæ.

After the enema she felt less full, having again parted with some flatus. Towards 9 o'clock, the pain returning in abdomen, I gave her her anodyne draught, and sent her—

R. Pil. galb. comp.	gr. v.
Ft. pilula, quartis horis sumenda.	

8th, 10 a.m.—A restless night, having talked a good deal; pain began again at 6; found her tolerably easy, but very depressed in spirits; abdomen not so full; no tenderness on pressure; less fever; pulse 140—145, but I think a good deal of this acceleration is due to mental disquietude; tongue clean; she vomits bile occasionally; was delirious in the night; the bowels

are very loose, the motions coming away in bed, without consciousness on the part of the patient. I, nevertheless, on account of the tympanitic condition, gave again a turpentine enema, which brought away a great quantity of flatus, &c. Left her at noon, expressing herself as feeling altogether better; abdomen very soft, and nothing like so full; bowels very loose; the lochia have ceased.

R. Confect. aromat.	℥ij.
Ammon. sesquicarb.	℥ss.
Tinct. opii	℥iss.
Spir. ammon. fætid.	℥ij.
Aq. menth. pip.	℥vj.

Sumat coch. ij., quartis horis, cum pil. galb. comp., gr. v.

Same day, 8 p.m.—Has had a few hours of refreshing sleep this afternoon, and feels better. No pain in abdomen, and very little distension or tympanitis; pulse 132. She is talkative and hysterical, inclined rather to cry than to laugh. Has been delirious in the course of the day, having fancies of the nature of *incubus*; fancying she was her own nurse, for example, and that she was lying in two separate pieces on the bed—one on one side and one on the other; insisting that the nurse, who had been rubbing her abdomen on one side of the bed, should go to the other side to rub the abdomen of the other body that lay there. Has some slight return of appetite. The mucous membrane of the tongue is abraded in patches.

Continue medicine, omit the night draught.

9th, 10 a.m.—Has had a good night. Complains of sore throat. It is decidedly *diphtheritic*. There are white patches on left tonsil and on uvula. Abdomen soft, full, and tympanitic. Is not correct in her ideas; is very cheerful. Still has some strange fancies, chiefly on the subject of her own personal identity. Never inquires about her children, or seems to care about them; naturally a very anxious mother. Pulse 132.

App. argenti nitr. tonsillis.	
R. Potassæ chloratis	℥j.
Quin. sulph.	gr. xij.
Acid. hydrochlorici	℥ss.
Tinct. ferri sesquichloridi	℥j.
Aquæ distill. ad	℥vj.
Sumat coch. ij. quartis horis.	

Same day, 8 p.m.—Pulse 120; throat worse; is much agitated and afraid of choking. I decided on staying all night. The throat is very full of viscid mucus, besides being diphtheritic in patches. I ordered her to take as much cold port wine as she

could get down. At twelve (midnight) she had vomited several times, and felt her throat much relieved, as well as her flatulent stomach. Pulse 110.

10th, 4 a.m.—Has been sick again; pulse 106; at 8, pulse 104; at 9, pulse 100. Complains of pains at scrobiculus cordis.

R. Quin. sulph., ʒss.
 Acid. hydrochlorici, ʒss.
 Aquæ ad, ʒxij. M.
 Sumat coch. ij. quartis horis.

R. Mellis, ʒj.
 Tinct. capsici, ʒj.
 Acid. hydrochlorici, ʒj.
 Tinct. myrrhæ, ʒj.
 Aquæ ad, ʒvj. M.
 Ft. gargarismus, sæpe utendus.

The application of nitrate of silver to the throat was repeated. The steady and equable dropping of the pulse made it obvious that along with the access of diphtheria a critical change for the better was taking place in the puerperal disease. The port wine ordered must, however, be allowed its part in contributing to this change; she drank altogether a bottle and a half in the course of the night.

Same day, 8 p.m.—Is better in every respect. Pulse 86. There is milk in the breasts.

11th—Going on well; throat still requiring the nitrate of silver; continue quinine.

12th—Throat troublesome; pulse natural.

13th—Throat still diphtheritic; continue gargle, quinine, and nitrate of silver dressing.

16th—Throat still diphtheritic, especially about the uvula; tongue bright, glazed, but moist—a small ulcer on it; is very weak; soft weak pulse, 88; appetite bad. Continue treatment.

17th—Pulse 88, stronger and firmer; throat and tongue the same.

R. Potassæ chloratis, ʒss.
 Quin. sulph, ʒss.
 Acid. hydrochlorici, ʒj.
 Aquæ pluvialis ad, ʒxij.
 Sumat coch. ij. ter die.

18th—Pulse 80. No diphtheritic patches visible. Is very feeble; occasionally vomits. Appetite bad.

19th—Pulse 96. Is sitting up. There has been some re-appearance of lochia during the last three days. Throat well;

still occasionally vomits bile. Ordered beer instead of wine. Continue medicine.

20th—Lochial discharge quite fresh and sufficient; is sitting up; pulse 90. The beer agrees.

23rd—Sitting up; pulse 68.

25th—Her recollection of all that has occurred is very dim and indistinct. She remembers, however, some of her delusions.

I have no further notes of this case. The patient improved gradually, but her convalescence was slow.

As a point of great interest, illustrative of the force of epidemics, I must here mention that a tradesman's wife at Alford, whom I was attending for remittent fever, *not puerperal*, which was complicated with asthenic pneumonia, had well-marked diphtheria, imported, so to speak, into the disease, as the pneumonic symptoms declined, exactly at the same time and in the same way as the preceding patient.

On the 4th of April I attended an easy quick multiparous labour, which was followed by considerable hæmorrhage; the subject of it, however, making a good recovery, without the occurrence of any bad symptoms during the month.

On the 9th of April I attended a similarly easy multiparous labour, which was followed by unusually severe after-pains. No bad symptoms during the month.

On the same day, in the evening, I delivered a woman of an anencephalous child which presented the back. The foetus having been forced down low in the pelvis before my arrival, I found the delivery difficult and troublesome. This woman recovered without any unfavourable symptoms.

April 11th—An easy, quick, natural labour; multiparous. No bad symptoms.

Same day I attended a very easy, quick, natural, multiparous labour. I had only time to make two vaginal examinations before the child was born. This patient had puerperal fever symptoms, followed by hysteria and melancholia, being the subject of Case IV. (page 10), which see.

13th—A rapid case of twins; the mother affected with excessive œdema of lower extremities, which soon disappeared after her confinement. Good and quick recovery.

CASE XLVII.—REMITTENT FEVER BECOMING A PUERPERAL FEVER;
DIPHTHERIA, &c. &c.

Mrs. T. of W., the wife of a farmer's foreman, who was in daily expectation of her second confinement, sent for me on the 13th of April, in the evening. She was not in labour, but I

found her in a high state of fever, with great headache; very hot moist skin; pulse 140; great thirst. She had had several rigors. In the next room there was lying ill in bed, a farm-labourer, a lodger in the house. He had, I found, fever, with some typhoid symptoms. I sent the woman a calomel aperient, and some simple saline fever medicine. The next morning she sent for me again. This time I found her in labour, and she was soon delivered, after a very easy, quick, natural labour, all her fever symptoms being quite in abeyance; skin cool; head quite free from pain; pulse 70, &c. &c. Being anxious about my lying-in patient, surrounded as I was by these nasty puerperal affections, I advised the removal to another house of the sick man, who, with typhoid fever, was lying in the adjoining room. He was removed, and died, poor fellow, three days after.

19th—Third day of lying-in. Fever returning. Continue saline medicine.

21st—Feverish; pulse quick and small; great dryness of mouth. Sumat quin. sulph. gr. ij., quartis horis.

23rd—Throat diphtheritic; pulse 90; tongue very much furrowed or cracked; its mucous membrane covered here and there with diphtheritic patches. I applied a strong solution of lunar caustic to the diphtheritic patches; continuing the administration of quinine.

24th—Pulse 96; throat still diphtheritic; tongue dry, glazy, and cracked; lochia suppressed two days; milk going; feverish heat of skin continues.

26th—Throat worse; pulse 104, smaller.

R. Potassæ chloratis,	℥ij.
Quin. sulph.,	℥ss.
Acid. hydrochlorici,	℥ss.
Aquæ ad,	℥xij. M.
Sumat coch. ij, quartis horis.	

I dressed the throat again with sol. arg. nitr.

After this date she recovered gradually, taking the medicine as prescribed above for two or three weeks.

On the 15th of April I attended a multiparous labour, lingering and slow; ergot given. This woman had some feverish symptoms during the first week, with the pulse over 100. She had some simple medicine, aperient and febrifuge, and her unfavourable symptoms soon passed away.

On the 16th of April, I attended an easy, quick, natural multiparous labour, the subject of which had a rapid recovery without any unfavourable symptoms.

On the 19th I attended a similarly good labour; no bad symptoms.

On the 26th and 28th also I attended similarly easy quick labours; no bad symptoms following either of them.

On the 2nd of May I attended a multiparous labour, which was slow and tedious, so that I gave a dose of ergot. No bad symptoms.

On the 3rd of May I attended two cases of labour, both multiparous; the former of which was slow and lingering, so that I gave ergot of rye; the latter quick and easy. No bad symptoms followed either of these cases.

On the 14th of May I attended a case of labour, multiparous, in which the face came round the pubes. I rectified the position with my finger, and afterwards, there being still some delay, I delivered with the vectis. No bad symptoms.

On the following day I attended an average primiparous case; ergot given. This woman was the subject of Case XV., reported at page 147.* I, therefore, omit the details of it here. There were rigors and fever, with abdominal tenderness, in the second week; the pulse 130, small, &c. &c. This state continuing until suppuration of the breast, in the fourth month, put an end to the symptoms by elimination of the *materies morbi*.

A hard primiparous labour, in which I accomplished the delivery with the long forceps, was attended on the 17th of May; no bad symptoms following.

On the 19th of May I attended an average, natural, multiparous labour; ergot given. No bad symptoms.

On the 28th of May I attended a case of placenta prævia, with twins, delivering both children by turning. No bad symptoms.

On the same day I attended an easy, natural, multiparous labour—the patient recovering without the occurrence of any unpleasant symptoms.

A similar case, attended on the 31st, had an equally favourable termination. But a second case attended on the same day, the 31st May, an average multiparous labour in which ergot was given, must come in here as—

CASE XLVIII.—INTERMITTENT FEVER.

I was summoned again to visit this patient on the 21st of June. I found she had had two shivering fits the day before, and one six days before that. She looked thin and ill; was feverish, with some perspiration, and the pulse 140, small and weak. The milk which had been abundant was now going. Her appetite was bad, and the tongue showed signs of irritation of the mucous membrane. There was no abdominal pain or tenderness.

* By mistake, Case XV. is described as having occurred on the 14th May; it ought to have been described as occurring on the 15th, as it stands here.

R. Quin. sulph.	gr. xx.
Infus. rosar	℥xij. M.
Sumat coch. ij, ter in die.	

27th—Is better; no return of rigors; pulse 100. Continue medicine. I heard no more of this patient until her next confinement took place about a year and a half after.

Four labours attended on the 1st, 2d, 9th, and 10th of June, respectively, one of which was primiparous, had all of them good recoveries without any bad symptoms.

June 15th—A labour attended on this day; easy, natural, multiparous, but complicated with considerable post-partum hæmorrhage, *ad deliquium*, was followed in the second week by anomalous febrile symptoms, terminating in phlegmasia dolens, after which the patient recovered rapidly. The details of this case having been already given in the group of cases illustrating the occasional occurrence of phlegmasia dolens, as seeming to constitute a favourable crisis in puerperal fevers (*vide* Case XXIX.), I do not repeat them here.

A woman, attended on the 20th June, after an easy natural multiparous labour; had a slow recovery, in consequence of hysteria, dyspepsia, headache, and other anomalous symptoms, including a preternaturally *slow* pulse, 60 in the minute.

Four labours attended on the 21st, 27th, 28th, and 29th of June respectively, one of which was a premature case of breech presentation; had, all of them, good recoveries.

Thus, then, of forty-eight cases of midwifery, attended personally by me between the 8th of March and the end of June, there were at least ten cases of puerperal disease sufficiently grave to excite some apprehension in my mind, when I reflected on the possibility of their having been caused by my attendance on the post-mortem examination of a woman who had died of acute puerperal peritonitis. Although, as I have already remarked, some of the cases, like that unfortunate one, exhibited inflammatory symptoms, they were all, equally like that case, adynamic in type, and were benefited by tonic and stimulant treatment. Of the four turning cases, only one was followed by unfavourable symptoms, and those symptoms were very brief—(Case XLIV.)—although the last, a twin case, presented the additional complication of *ante-partum* hæmorrhage from *placenta prævia*, and I had used some force in passing my hand through the os uteri. Of the instrumental cases, four in number, only one was the subject of disease of the puerperal state—(Case XLVI.) The fact that before entering on this narrative of this epidemic of puerperal fever in my practice, I had already reported the details of several of the cases under other groups in the present paper, furnishes

an argument strongly corroborative of the doctrine with which I commenced, viz., That although we must regard the different febrile diseases which occur in the puerperal state as being *one* disease, yet that that one disease presents itself with many varying manifestations.

Before passing on to another group of cases, I feel that the narrative above given will scarcely be complete without some account of the cases attended in my practice during the four months next following. In brief, then, I may state that during that period I attended 52 cases of midwifery, including 19 with the following special circumstances, viz., 6 deliveries with the forceps; 2 with the vectis; 4 by turning; 2 cases of incarcerated placenta; 3 with still-born children, of which 2 were putrid; 1 case of breech presentation; and 1 of twins. And as these 52 cases, with two exceptions, recovered without the development of any puerperal febrile affections, I felt that I could reasonably believe that the contagion or epidemic influence, or whatever it might have been that had produced the alarming series of diseases above described, had ceased with the end of June. One of the excepted cases, attended on the 15th August, being one of those complicated with incarcerated placenta, was the case of *diarrhœa*, terminating in purulent elimination from the breast and lungs—already described in detail in this paper (Case XIII.) The other case, attended on the 30th of August, an easy labour, with second child, was followed on the third day by a smart attack of ephemeral fever, during the reactive stage of which the pulse was 180. Both these patients ultimately did well.

I now propose to present reports of cases illustrative

OF THE INFLUENCE OF PARTURITION AND THE PUERPERAL STATE ON PRE-EXISTING DISEASE.

Although, as will be seen in some of the following reports, parturition and perhaps the first day or two of the puerperal state have occasionally a remarkable influence in interposing a temporary check to the severity of a pre-existing disease, perhaps on some principle of counter-irritation; yet this same puerperal state usually in a short time asserts its pre-eminence, and we find many of the characteristic phenomena of puerperal fevers imported into the original malady. See Case VIII. reported in the pyogenic group, and Case XLVII. reported in the epidemic group; the former a case of dysenteric diarrhœa commencing some days before the premature labour which it induced; the latter, a case of remittent fever commencing a few days before natural labour at the full period. My notes of the first case in this group are somewhat scanty:—

CASE XLIX.—CHRONIC BRONCHITIS.

Mrs. W. H. of H., who was expecting her confinement to take place in about three months, applied to me on the 1st January, 1840. She had been suffering from bronchitis for several months, the disease having become considerably aggravated as winter advanced. She improved somewhat under the treatment adopted, which consisted in a little local depletion, blisters on the chest, antimonials, &c. But labour came on on the 23rd January, which was about six weeks before the full period. The labour was natural and easy—multiparous. For a few days after she was considerably better, with the pulse below 90, which had previously for the most part ranged between 120 and 140, the milk appearing in the breasts, and the lochial discharge being sufficient. But on the fifth day, after over-exerting her lungs with talking, she was taken worse; her pulse rising to 130, with an aggravation of the cough and hectic symptoms. Counter-irritation and other remedial means were adopted for her relief, but she finally sank on the 12th February in the night, having survived her labour about three weeks.

CASE L.—GASTRITIS; DIARRHŒA.

E. W. of C., an unmarried woman, was confined of her second child, March 11th, 1848. She had been suffering from diarrhœa and vomitings for about a month before her confinement, and I had not been informed of it. Her labour was hard and tedious from rigid os uteri. When I called upon her on the third day, she was doing very well, every thing natural and right; parturition having perhaps checked the disease. But on the 23rd of March, I was requested to see her again—the 13th day of lying-in. I found her very bad. She had had a shivering fit that day week, followed by fever, headache, and a return of diarrhœa with irritability of stomach; this state continuing until the following Monday, March 20th, when she had another rigor followed by an aggravation of the symptoms above mentioned. These continued until the morning of the day when I was sent for, when she had a third rigor. When I thus saw her, her pulse was 145, rather hard and small; she was continually vomiting; the skin hot and feverish; there was headache, with some confusion and delirium; slight abdominal pain, with tenderness; lochia not suppressed; milk scanty; the bowels had been sometimes loose, sometimes bound; she had taken castor oil.

Applicetur empl. canth. epigastr.
Sumat calomelanos gr. j.; opii gr. $\frac{1}{2}$ quartis horis
in haust. effervesc.

24th—Sickness abated; pulse 128, very weak; all other symptoms better.

25th—Pulse 120, very weak; no vomitings; bowels too loose. To discontinue the calomel and take 5 gr. doses of carbonate of ammonia with aromatic confection and laudanum every four hours, together with a pill containing a grain of Quin. disulph.

26th—Pulse 106, firmer. Better altogether. Continue.

27th—Worse again; tongue dry; pulse 120, feverish; bowels not moved since 25th; sick again; headache; no abdominal pain. Gave effervescent again with calomel and opium as before.

28th—Another rigor last night; feverish; pulse 130; head confused; vomits frequently. Continue medicine.

29th—Slight ptialism; pulse 120, feverish; to discontinue the calomel and opium, but go on with the effervescent.

30th—Pulse 120; fever of hectic type; there is much cough with expectoration; clammy sweat; some delirium; she still vomits occasionally. She continued in nearly the same state until April 4th, during which time I was giving tonics, and occasionally cordial astringents when diarrhoea was urgent.

April 5th—On this day, in spite of my advice, she was taken down stairs, and sat up for nearly three hours, when she fainted. I was summoned, and found her with a countless sinking pulse, ghastly face, clammy sweat, &c., although there was no hurry of respiration. I ordered stimulants and cordials to be administered *ad libitum*. The next day she had rallied somewhat, this improvement maintaining itself until April 8th, when I found the pulse 120, firm and steady; but the following morning, April 9th, she fainted again and died exhausted, having survived her confinement a month and a day.

CASE LL.—DYSENTERY.

October 16th—I was summoned to attend on Mrs. J. R. of B., the wife of a labourer. On my arrival I found that the child had been born an hour, and that I should not have been sent for at all, if the women present had been able to remove the placenta, which was adherent. It was the second confinement. I learned on inquiry, that the woman had been suffering from dysenteric diarrhoea for several weeks, and that the child had been unexpectedly expelled while she was straining at stool. I had no difficulty with the placenta. I gave her an opiate and left her, pulse 100. The next day she was comfortable; pulse 90; no return of purging. But on the fifth day—

29th—An attack of shivering followed by fever and pain in abdomen; countenance anxious; pulse 145, small and sharp; lochia scanty; no milk. To foment, and take *mist. puerp.*

21st—Fever; pain; diarrhoea; pulse 140. To take sulphate of copper gr. $\frac{1}{4}$ with opium every four hours, in infusion of calumbo.

22nd—Better; pulse 115. Continue.

23rd—Dysenteric diarrhoea returned last night; pulse this morning 112; diarrhoea frequent. At noon, a severe rigor followed by fever and a pulse of 150; diarrhoea checked, having been taking her copper pills since the morning in decoct. quercus. She is very thirsty.

24th—Great agitation; hurried respiration; hot skin; flushed face; abdomen full, soft, not painful, but very tympanitic; pulse 180.

R. Spi. ammon. foetid.	3j.
Tinct. opii	3ss.
Mist. camph.	3iss. M.
Ft. haustus statim sumendus.	

7 p.m.—Is relieved by the draught; agitation gone; pulse 120.

25th a.m.—Diaphoresis present; pulse 140, full, soft; p.m. the diarrhoea has returned profusely. To take a copper pill with each dose of the following:—

R. Tinct. opii	3ij.
Quin. sulph.	gr. xij.
Tinct. valer.	3ss.
— zingiberis	3ij.
Infus. rosar. ad	3vj. M.
Sumat coch. ij.	quartis horis.

26th—She died this morning at 10, after suffering great pain.

CASE LII.—REMITTENT FEVER; DIARRHOEA; IRRITABLE STOMACH.

March 23rd, 1857—I was engaged to attend Mrs. T. M. of T., who expected to be confined in about two months. She was the mother of a large family, and was at this date suffering from headache with some feverish symptoms, and a cough with pain at sternum. I gave her a few doses of tinct. camph. comp. in bitter infusion. I saw her the next day, and found she had *low fever*. Her bowels being somewhat confined, I gave her—

R. Magnesiae sulph.	3j.
Pulv. ipecac. comp.	3ss.
Spir. æth. nitric.	3ss.
Liq. ammon. acet.	3ij.
Mist. camph. ad	Oss. M.
Sumat coch. ij.	quartis horis.

26th—The feverish symptoms are unabated; applied a small blister to the sternum, and the bowels being sufficiently open, I substituted ℥ij of magnes. sulph. for the ℥j. prescribed on the 24th.

29th—Fever; occasional shiverings; headache; furred slimy tongue (Query, gastric irritation?); pulse 140.

R. Quin. sulph.	gr. xvj.
Spir. æth. nitr.	℥ss.
Infus. rosar. ad	Oss. M.
Sumat coch. ij.	quartis horis.

and to take 4 grs. of blue pill at bed-time for three nights.

30th—Is somewhat better; pulse 96. Continue.

April 1st—Feverish symptoms continue; tongue brown and dry in centre; pulse 80. Continue medicine.

2nd—Aphthæ in mouth; pulse 90. Continue the medicine; to have a lotion for the mouth, composed of borax and honey.

3rd—Pulse 96. Fever, with diarrhœa and sickness; mouth better.

R. Magnesiæ calcinatæ	℥ss.
Pulv. tragac. comp.	℥j.
Acid. hydrocyanic.	(Scheele) mxx.
Tinct. card. comp.	℥ss.
Aquæ ad	Oss. M.
Sumat coch. ij.	quartis horis.

5th—I was hastily summoned early this morning, the patient having been taken in labour. On my arrival, distance seven miles, I found she had just given birth to a living boy. She was in a very good state, every untoward symptom being in abeyance; and after removing the placenta, I left her feeling very comfortable, with the pulse 72 in the minute.

6th—There is some return of febrile symptoms; she has no appetite, and the abdomen is very tympanitic; the pulse 96, but soft and favourable; complains of flatulence; no pain.

R. Pil. galb. comp. ℥j. divide in pilulas xvj.; quarum sumat ij. cum coch. ij. misturæ sequentis.

R. Spir. ammon. aromat.	℥vj.
Tinct. rhei. comp.	℥ss.
Tinct. gent. c.	℥ss.
Aquæ ad	Oss. Ft. mistura.

8th—Pulse 112; has been very sick; there is great tympanitis; slight looseness of bowels. To take again the medicine prescribed on the 3rd with the addition of aromatic confection in doses of 12 grs. each.

10th—Pulse 110; diarrhoea profuse; no appetite; tongue clean; there is great debility; some milk in breasts; lochia sufficient.

R. Plumbi acet.	℥j.
Acid. acetic.	℥ij.
Tinct. opii	℥ij.
Tinct. catechu	℥ss.
Aquæ menth. pip. ad	Oss. M.
Sumat coch. ij.	quartis horis.

11th—Pulse 106; she is better; diarrhoea checked; is not so sick; tongue clean; tympanitis better; some milk; lochia right.

15th—Pulse 115; she is not so well; diarrhoea returning; the vomitings are frequent and troublesome; there are aphthæ in the mouth again; appetite gone. To take again the lead mixture, and a pill containing one grain of quinine with each dose of it. To use the borax lotion again.

17th—Pulse 104; face flushed; emaciation; diarrhoea and vomiting going on. To take sulphate of copper and quinine in pills.

18th—Pulse 120; diarrhoea as before. Continue.

19th—Pulse 120; diarrhoea checked. Continue.

20th—Pulse 125; diarrhoea as bad as ever. To discontinue copper and quinine, and take chalk mixture with aromatic confection and laudanum.

21st—Diarrhoea continuing, I gave her—

R. Tinct. opii	℥ij.
Spir. ammoniæ arom.	℥ss.
Decoct. quercûs ad	Oss. M.
Sumat coch. ij.	quartis horis.

22nd—Better altogether; pulse 100. Continue.

24th—The vomitings are now the most troublesome symptom, consisting chiefly of large quantities of transparent fluid—a secretion from the coats of the stomach probably, for she vomits more than she takes. The pulse 120, and emaciation great; the diarrhoea is still troublesome.

R. Bismuthi trisnitr.	℥ij.
Pulv. acaciæ	℥ij.
Tinct. card. comp.	℥ss.
Aquæ ad	Oss. M.
Sumat coch. ij.	quartis horis.

27th—I found it advisable to give decoctum quercûs again, continuing the bismuth, which was checking the vomitings.

After this she recovered, but very gradually; she was for a

long time hectic and emaciated. She took the bismuth for several weeks, and quinine in pills. I was at one time afraid she would sink from exhaustion. The case altogether gave me a great deal of trouble and anxiety. The child lived and thrived, although it suffered a long time for the want of the breast.

CASE LIII.—DIARRHŒA.

Mrs. P. H. of M., near Alford, in the eighth month of her first pregnancy, was attacked with profuse diarrhœa, and when I saw her on the 25th of April, 1861, I found her in a bad state, with a very rapid pulse, profuse purgings, nausea at stomach, great pain, fever and shiverings. I gave her some chalk mixture with aromatic confection and laudanum, and felt some apprehension regarding the possible consequences of labour coming on before the abatement of the diarrhœa. I was, however, summoned early the following morning and found her in labour, and the diarrhœa somewhat checked. I delivered her of a premature living boy, and left her feeling very comfortable; but this lull was of very short duration, for the diarrhœa returned two hours after delivery, with faintings. At 11 a.m., when I saw her, the pulse was 140, full, reactive; the skin was warm and perspiring; there was very great flatulence of stomach. Sumat statim ol. terebinth. ℥j. ex lactis ℥ij., and to recommence the chalk mixture.

April 27th—Diarrhœa not quite so frequent, recurring, however, every two hours; the motions less liquid; no appetite; pulse 125; skin hot; slight tenderness of abdomen and fundus uteri. To foment, and continue medicine.

28th—Diarrhœa every three or four hours; fever; there is less pain and tenderness of abdomen; continue. She recovered steadily after this, so that *nous en étions quittes pour la peur*. The child lived about a week.

The last case reported I look upon as exceptional in some respects. The duration of the abatement of the symptoms caused by the access of the labour was shorter than in the other cases, as was also that of the *puerperal* complication.

Perhaps the most striking illustrations of the influence of parturition in checking and controlling a pre-existing disease may be found in certain cases of convulsions. There is, it appears to me, a distinct class of *ante partum* convulsions, occurring before any symptoms of labour are present at all. These convulsions would appear to depend not so much on uterine irritation, as on a disturbed state of the system generally, interfering with, and hindering the establishment of, the parturient process, rather than co-operating with it or accompanying it in its progress; a kind of misplaced nervous influence, so to speak. Or perhaps it would

be more correct to assume that, while in one class of cases the source of the irritation is in the uterus itself, in this class the irritation originates elsewhere; so that the attention of the nervous influence is altogether diverted, if I may be allowed such an expression, from the object to be accomplished. The convulsive state is really substituted for the healthy action of labour; the latter process not going on at all, until the former condition is removed; or, conversely, the convulsive action not ceasing, until healthy uterine action is brought on.

The following cases appear to me to confirm this view of the subject:—

CASE LIV.—CONVULSIONS.

On the 6th of April, 1853, one of my medical confrères consulted me respecting the case of a woman who had engaged him to attend her in her first confinement. She was an epileptic, and during the previous fortnight her fits had gradually increased in frequency and severity. She was now at the end of her reckoning, and the intervals between her fits had become very short indeed. I advised bleeding, as the fits, from Mr. ———'s description, resembled puerperal convulsions. He bled her, and then took me to see her. She had a short fit while I was in the room; but it was much less severe in character than many of the previous ones. Finding that there was no uterine action going on, though the os uteri was dilated to the size of a shilling, the membranes feeling lax, and the head floating loosely above the pelvis, as well during a fit as in the absence of one, while not the slightest appearance of pains had otherwise shown themselves, it struck me that if genuine uterine action could be induced by mild means, the convulsions would cease; at any rate, that an end would be put to this state of suspense and anxiety by the poor woman's delivery. I therefore advised Mr. ——— to rupture the membranes. This was done at half-past 6 p.m. Natural pains came on during the night, and she was safely delivered the next morning at eight of a still-born child, apparently but just dead. She had no fits of any sort after the rupture of the membranes, and got on well.

CASE LV.—CONVULSIONS.

On the 3rd of March, 1854, I was sent for to attend a woman in a village six miles from Alford, who had bespoken me for her expected first confinement. She was a tall young woman, with a very pale complexion, aged 22. On my arrival at 3 p.m., I learned that she had had strong and very frequent pains all the day. She was at the full period of gestation. On examination I found the os uteri perfectly closed and puckered up; a wide

pelvis, with the head resting on a well-developed cervix. Concluding that they were false pains, I gave her a full dose of laudanum; and being told that the bowels were in a constipated state, I ordered her to take an ounce of castor oil in the evening. The laudanum quieted the pains at once, and I left her.

I was sent for again the following day, and arriving at half-past 3 p.m., I found that she had had four strong fits of convulsions; the first, at half-past 11 a.m.; the last, about half an hour before my arrival. She was semi-conscious, and, when roused, complained much of pain in her head. The os uteri was in the same state as on the day before, and she had had no pains since taking the laudanum. She had taken the castor oil, but it had made her sick, and she had been vomiting at intervals, all night, bilious-looking stuff in large quantities. The bowels had not been moved. The pulse was nearly natural. I bled her in the arm immediately, taking at least thirty ounces from a large orifice, when she began to complain more and more of her head. I asked if it was a faint sensation. She said she hardly knew; it was something very bad. I tied up her arm, and presently a dreadful fit came on—the countenance livid and hideously distorted, and all the muscles of the body convulsed. I examined *per vaginam* during the fit, and found the head pressed tighter into the pelvis, but the os uteri just the same. I forced a finger through the os, passed it round for some space between the membranes and uterus, and then ruptured the membranes, thinking it desirable to induce labour pains. The liquor amnii began to escape forcibly, evidently from the strength of the universal muscular contraction present. I then, when the fit had in about ten minutes subsided from its extreme violence, took about twenty ounces of blood more. The breathing was very stertorous, and the patient quite comatose for some time. When the fresh quantity of blood began to flow, it was quite black; but, as semi-consciousness returned, it improved in colour. I next applied a large mustard poultice to the back of the neck, gave a powder containing ten grains of calomel and twenty of rhubarb (from the medicine chest of a lady residing near), and then administered an enema of salt and gruel. This acted immediately; the patient jumped out of bed, and voided an immense quantity of dark-coloured fæces, declaring that her head felt quite right again. I waited three hours from the time of the last fit; and then, finding the patient quite relieved, with the countenance and pulse natural, and the os uteri still rigid and indisposed to open, although she had had two or three slight pains, I returned home, leaving orders to be sent for again if the slightest symptoms of convulsions should reappear, or if the pains should rapidly improve in quality. This was about 7 o'clock. At 11 I

received another message. I hastened down to my patient's residence, and found that she had been safely delivered at a quarter past eleven, of a still-born child, apparently but just dead, after only about half an hour's active pains, the labour having been a more than usually easy one for a primipara. She appeared perfectly well, had a nice loss, was rather sleepy, had no headache, and but very slight acceleration of pulse. I was told that, after I left her, she had continued to have slight pains at long intervals until the last three quarters of an hour or thereabouts, when they became all at once quick and strong. She had no recollection of her fits, or of having been bled, or of my having been with her at all; but she remembered the birth of the child. This woman recovered very rapidly and perfectly; and, notwithstanding she was thrown into a state of great agitation and distress within the week, in consequence of her husband having had his pocket picked at Lincoln of a little legacy of £35, which he had gone there to receive, she had not the slightest symptom of a convulsion after.

In this case, at the full period of gestation, we had derangement of the stomach and bowels, causing such irritation in the whole system as to lead to false pains at first, and ultimately to convulsions. It seems clear to me that it was this derangement that prevented the establishment of the parturient process, and that the inability of the nervous system to set up that process led to the convulsions. The bleeding was probably useful in relieving the induced congestion; but I think that the enema was the principal agent in the cure, by removing the original exciting cause of the disturbed balance of nervous influence; while the rupture of the membranes materially assisted by directing that nervous influence to the proper work which it had to perform.

I have many times found a similar advantage from rupturing the membranes before dilatation of the os uteri, when, for some inexplicable reason, the uterine action, evidently impending and causing all sorts of irregular pains and disquietude, has still refused to come on properly; the labour has gone on correctly at once. But this *par parenthèse*.

CASE LVI.—CONVULSIONS; ALBUMINURIA.

In the present case the cessation of convulsions was not so complete as in the last, there having been one fit during the labour—in its last stage:—

Mrs. R. S. of Alford, the wife of a bookseller, aged 36; married 8 years, being for the first time in the family way, and expecting her confinement in about a month, was attacked at 5 a.m., December 31st, 1857, with bilious vomitings, which continued all the morning. I saw her at 2 p.m. She complained

much of headache, and her pulse was slow and weak. I gave her calcined magnesia with two-minim doses of Scheele's hydrocyanic acid. The vomitings ceased at 3; the headache, however, continuing. At half-past 5 she had a short fit of convulsions, and presently another and another, which last I saw. The pulse was rapid, the pupils dilated, the right more so than the left; the os uteri thin and very rigid, just admitting the tip of the forefinger; head in first position. I ruptured the membranes, bled her in the arm to 24 ounces; gave a turpentine enema, and applied a sinapism to the nape of the neck. I suspected albuminuria, the face being puffy and œdematous; but there was no urine forthcoming for examination. After a copious feculent motion, and when the sinapism began to redden the skin, at about 7 the convulsions ceased, and she fell into a calm sleep without stertor, the sleep between the fits of convulsions having been stertorous. The pulse dropped to about 80, soft and pleasant. At 10½ p.m. no more fits; os uteri dilating nicely, and she had regular pains. I was summoned at half-past 4 a.m. Found the os uteri fully dilated, with good pains going on; the foetal head well in the pelvis. She had had no more fits. The pupils were now contracted, one being as at first more dilated than the other. While I was making an examination, she had a strong pain; but during the next pain which speedily followed, a strong fit of convulsions came on. I therefore immediately cut the matter short by delivering with the vectis. The child was living. She presently fell asleep again; and I left her about an hour after still asleep calmly, with a soft pleasant pulse 84 in the minute. She had no more fits, and made a rapid recovery. While the convulsions were going on, she was occasionally between the fits, if not stertorously asleep, very hysterical and unruly. The urine passed in the course of the day I found to be highly albuminous, and of sp. gr. only 1008. The next day all the albumen had disappeared.

OF THE INFLUENCE OF A MORIBUND CONDITION OF THE MOTHER
DURING ADVANCED PREGNANCY IN INDUCING EXPULSION OF
THE FETUS BEFORE DEATH TAKES PLACE.

Although, perhaps, not strictly within the limits of a series of reports illustrative of diseases of the puerperal state, I, nevertheless, think it may not be altogether irrelevant, if I venture to introduce in this place my notes of two cases, which serve to show that when a woman is on the point of death from disease occurring during her pregnancy, the foetus will usually be expelled either dead or alive, most frequently perhaps dead, before the mother dies.

CASE LVII.—CYNANCHE TONSILLARIS; BRONCHITIS; PNEUMONIA;
DIARRHŒA.

On the 4th of July, 1840, I was requested to visit Mrs. L. B. of S., the mother of a large family, and a very robust sanguineous-looking woman. I found she had cynanche tonsillaris. I scarified the tonsil freely, and sent her a gargle, with some aperient medicine. I was sent for again in the evening, and found her considerably worse; the inflammation, it would seem, having extended to the bronchi, for there was more difficulty of breathing than the state of the tonsil would account for, and there was mucous cough, mucous râle, and pain in the chest. I bled her largely in the arm, and ordered steam inhalations, &c. The blood was very buffy. The next day she was in every respect better, and she then bespoke me for her confinement expected in three or four months. The next day, Monday, in the morning, she sent me word that she felt so much better, that I should have no need to visit her that day. In the evening, however, I was fetched again in haste. I found she had been talking and exciting herself very much all day, it being the village feast; and had, moreover, taken some ale, some gin, some laudanum, about ℥j., some "Bateman's drops," some tincture of rhubarb—all with a view to the removal of the gradually increasing pain she felt in the chest. I found, in short, that pneumonia had set in. I bled her again, and applied a large blister to the chest. The blood was buffed and cupped. The difficulty of breathing was very great; the bleeding and blistering afforded no relief; and as she turned cold frequently, with a feeble rapid pulse, I soon found I could not go on with the $1\frac{1}{2}$ gr. doses of tartarized antimony I had commenced with after the bleeding. On Tuesday morning, the bowels being confined, I gave two or three doses of castor oil, which failed to act. In the evening I gave her a turpentine enema. There being no effect, in two hours I gave her another, which acted well. I was called up quite early the next morning with a message that she had had diarrhœa all night. On my arrival, I found her just delivered of a six months' foetus, which never breathed, although its heart continued to beat full twenty minutes after its birth. I removed the placenta. The poor woman was in a very bad state, gasping for breath, and purging profusely, with a sinking pulse and cold skin. I gave her some ammonia, with hot wine and water; but she expired about two hours and a half after the birth of the child. She had felt no pain in the chest for more than twelve hours before her death; the labour pains having probably taken the place of the chest pain.

CASE LVIII.—SPASMODIC CHOLERA.

Mrs. W. G. of S., the wife of a farmer, bespoke me early in March, 1842, for her confinement, expected to take place in the following May. On the 17th of March I was sent for early in the morning. I found her suffering from some of the symptoms of "English cholera," namely, bilious vomitings, with coldness of surface and cramps. The bowels were constipated. As the stomach seemed foul, I gave an emetic, to be followed by some alterative doses of blue pill with saline effervescent. When I called the next day I found the vomiting still going on without any abatement. The bowels had acted. I applied a sinapism to the pit of the stomach. I sent some more alterative doses, and continued the effervescent, not anticipating any danger. The next morning I was sent for early, and found profuse diarrhoea superadded to the other symptoms, with a very rapid pulse and anxious countenance. I ordered brandy, and gave her some astringent medicine with aromatics. At her request, I examined her to ascertain if labour was coming on. The os uteri was closed and pursed up. In the afternoon, however, a man came full gallop to tell me that her child was born. I went in hot haste; distance six miles; and found that she had indeed been delivered of a seven months' still-born child. I found the after-birth in the bed. She was now moribund, with a rapid sinking pulse, hurried respiration, &c., and died about two hours after.

I look upon the occurrence of diarrhoea in the two cases given above, especially in the former of them, as highly characteristic. Indeed, I am inclined to think, that if there be any one symptom more than another characteristic of puerperal disease, that symptom is diarrhoea.

OF DYSURIA AS A PUERPERAL DISEASE.

It will have been noticed that retention of urine was a prominent, and more or less troublesome symptom, in several of the cases of puerperal fever already reported.—(See Cases VIII., XXXIX., XL., XLI., XLII., XLIII.) But it is not merely as a symptom in puerperal fevers that retention of urine is met with, for it occasionally occurs as an independent disorder of the puerperal state; a disorder too, which is exceedingly liable to recur in the same patient. I subjoin my notes of a few cases in illustration of this troublesome and painful affection.

CASE LIX.—DYSURIA.

March 3rd, 1841—Multipara. An easy labour. In consequence of retention of urine, catheterism was necessary once,

about twelve hours after delivery. She took no medicine, and a repetition of the operation was not required.

CASE LX.—DYSURIA.

February 18th, 1843—Third child. An easy labour—the subject of the last case. Catheterism again necessary, twelve hours after delivery. As on the previous occasion, a repetition of the operation was unnecessary.

Thus, in these two cases, we find a woman suffering from precisely the same affection after two successive labours, her second and third. From her first, a hard and troublesome case of breech presentation, she recovered without an untoward symptom of any kind.

I afterwards met with the same kind of recurrence of dysuria in another patient, whose cases I give below:—

CASE LXI.—DYSURIA.

May 3rd, 1845—Multipara. An easy labour. After-pains very severe on the second and third days. Used fomentation and anodynes. Catheterism necessary in the morning of the fourth day, to relieve a retention existing from the evening of the second day. The pain was violent indeed, and aggravated in paroxysms like the after-pains. Pulse not affected. Catheter used three successive mornings. The tinctura ferri sesquichloridi put an end to the retention after the third catheterism.

CASE LXII.—DYSURIA.

July 20th, 1848—The subject of the last case. An easy labour. After-pains particularly severe, with complete retention of urine. Doses of pulv. ipecac. comp., with ʒj. doses of sp. æth. nitrici, acted beneficially, and catheterism was unnecessary.

This woman, I was told, in her next labour, in which she was attended by another practitioner, had the same symptoms again, which were relieved by catheterism.

CASE LXIII.—DYSURIA, DIARRHŒA.

March 20th, 1851—Multipara. Easy labour. Retention of urine during the first twenty-four hours after delivery, causing severe pains. It was relieved by liq. potassæ with laudanum and mucilage. She was after this teased with diarrhœa for two or three weeks, for which she had chalk mixture with laudanum, tinct. catechu, and aromatics.

CASE LXIV.—DYSURIA.

June 17th, 1855—Multipara. An easy labour. There was complete retention of urine in this case, requiring catheterism every day until the 24th June, when a few doses of tinct. ferri sesquichloridi removed the affection, after the failure of demulcents with laudanum and liq. potassæ.

Fearing a recurrence of dysuria with this patient after her next confinement, November 18, 1856, I gave her the following medicine soon after she got it over, and she had no dysuria—

R. Magnesiæ sulph.	ʒj.
Tinct. opii	ʒij.
Spir. æth. nitric.	ʒj.
Tinct. card. comp.	ʒss.
Mist. camph.	ʒvss. M.
Sumat coch. ij. quartis horis.	

There would appear to some capriciousness about this troublesome affection, dysuria, when it occurs as a puerperal malady. Its liability to recurrence in the same patient is well shown in the four first cases reported here. With the subjects of the two last cases given here there was no recurrence. They have both had several children since. My cases clearly show this also—that the medicine which will cure or relieve in one case, may altogether fail in another; a peculiarity noticeable in some of the instances in which I have described dysuria as a symptom in puerperal fever.

OF CERTAIN MISCELLANEOUS AFFECTIONS OCCURRING IN THE
PUERPERAL STATE.

CASE LXV.—INCARCERATION OF LOCHIAL DISCHARGE.

July 29th, 1838—Primipara, aged 38. This woman had a very narrow pelvis, and was, moreover, very unruly and unmanageable. The child's head was very large and bony. When she had been many hours in labour, the head making no progress, I applied the forceps, but failed with it. I then introduced the perforator, and breaking up the head, succeeded in the delivery after much difficulty, until I came to the shoulders; they would not follow, so I at last fixed the blunt-hook in one of the armpits, and thus, with very powerful traction, I brought away the body.

The next day, in consequence of the swollen state of the vagina and labia, added to the coagulated condition of the lochia, that discharge was entirely prevented from escaping. The abdomen

swelled very much, and the woman perspired excessively; but she had no bad symptoms, the pulse remaining about 90, soft and full, and there were no shiverings. Fomentations were used diligently, both to the abdomen and the vulva—to the latter with warm Goulard lotion; and the bowels were kept open. On putrefaction of the coagula taking place, and the excessive swelling of the soft parts diminishing, there was an abundant discharge of very black and offensive stuff; the abdomen at the same time diminished in size, and the woman had a speedy recovery.

The case given above was clearly one of *traumatism*, and yet there was no puerperal fever. The following case, unimportant in itself, serves at least to show how a condition in some respects resembling that case may simulate a puerperal fever:—

CASE LXVI.—INCARCERATED LOCHIAL DISCHARGE.

December 18th, 1838—Multipara. An easy labour. Calling at the end of 36 hours, I found the after-pains excessively severe; the lochial discharge obstructed by coagula; pulse 120, full and soft; skin hot, but moist; no tenderness of abdomen. Gave a dose of ol. ricini, followed by anodynes and fomentations. The lochial discharge escaped and all went on well.

CASE LXVII.—HYSTERALGIA.

November 30th, 1838—Multipara. Very easy labour. Was attacked on the third day in the morning quite suddenly, with violent uterine pain, preceded by slight shivering; skin hot, pulse 120 and small; slight tenderness on one side of abdomen. A dose of ol. ricini with fomentations removed the more urgent symptoms. The next day she had sweated a little; no pain, tenderness as before, slight fever, pulse 115. Applied half a dozen leeches to the tender part. The lochia was slightly obstructed. The next day she was quite well. Burns describes an affection something like this case, under the term *Hysteralgia*.

CASE LXVIII.—SECONDARY HÆMORRHAGE.

December 13th, 1837—An average labour. Multipara. The woman appeared to be doing quite well until the evening of the 28th December, the sixteenth day after delivery, when I received a hasty summons. She was sitting against the kitchen fire, when suddenly she was alarmed by a great gush of blood from the uterus. I found her in bed on my arrival, with the usual symptoms attending profuse hæmorrhage. I stayed with her two hours, during which time I gave her two large doses of acetate

of lead and opium, and applied the cold douche. I afterwards sent her the following :—

R. Plumbi acet.	gr. xvj.
Liquor opii sedat.	ʒj.
Acid. acetic. fort.	ʒj.
Aquæ menth. pip. ad	ʒviij. M.
Sumat coch. ij. ampl. tertiis horis.	

There was no return of hæmorrhage, and all went on well.

CASE LXIX.—SECONDARY HÆMORRHAGE.

This case was not so fortunate as the last, as will be seen. There was, indeed, an element of puerperal fever in it, which would probably contribute its share to the fatal issue. Shall I add that there was also a *traumatism*? For, although the os uteri was fairly dilatable, yet, as it was nearly closed, some *little* force was of necessity used in passing the hand through it for the purpose of turning.

February 3rd, 1860—Multipara. Presentation of a hand with vertex. Premature. Labour pains had been going on for twenty-four hours with but little effect on the os uteri, which remained nearly closed though dilatable. Delivery by turning in the evening; the os uteri readily giving way to the introduction of the hand. Child living. One hour after delivery there was a full reactive pulse, 120.

4th—She has had a great deal of hæmorrhage; the pulse can scarcely be felt. Expresses herself as feeling in other respects pretty comfortable.

5th—Pulse 120, small; no abdominal pain or tenderness; abdomen soft, rather tympanitic; is sick occasionally; bowels confined; no appetite. To take half an ounce of castor oil.

6th—Pulse 125, small; the bowels have been moved, but not sufficiently. There is considerable *malaise*. To take mist. puerp.

7th, evening—Pulse 140, thin and small; feels “sore all over;” there is still *malaise*; cheeks flushed; tongue clean; is frequently sick; bowels have been well moved; lochia and milk sufficient; abdomen soft and full; no pain or tenderness; no appetite. To take quinine in small doses.

8th—Sixth day of lying-in. A sudden attack of profuse hæmorrhage came on this morning, which was arrested by cold applications, and gallic acid taken internally, with brandy to relieve the faintings. She rallied in the middle of the day, having taken a good deal of brandy. The hæmorrhage did not

return; but, notwithstanding, collapse came on again, and she died exhausted in the evening.

CASE LXX.—RHEUMATIC FEVER.

February 2nd, 1844—Multipara. Easy natural labour. She caught cold about the ninth day, and had an attack of rheumatic fever. I saw her on the fourteenth day. High fever; pulse 140, full and soft; skin moist; great pain in head; some delirium; lumbago, and "pain all over;" lochia and milk suppressed; no appetite; tongue white, with red edges; some tenderness at epigastrium, none lower down; bowels confined. Sumat statim calomelanos gr. v., et pulv. jalapæ comp. ℥ss.

R. Tinct. sem. colchici	℥ij.
Pulv. ipecac. comp.	℥ij.
Magnesiae bicarb.	℥ij.
Spir. ætheris nitrici	℥ss.
Mist. camph. ad	Oss. M.
Sumat coch. ij. quartis horis.	

Next day—Pulse 90; headache nearly the same; no delirium; still pain at scrobiculus cordis; the pain in the loins and joints is better. The bowels have not been sufficiently relieved. Sumat ol. ricini ℥vj., et pergat in usu medicament.

Next day—Feels nearly well; the pulse 70. The milk had not returned when I saw her three days later, although she was doing quite well in other respects.

CASE LXXI.—PLEURISY; DIARRHŒA.

This case, which is the last on my programme, might have been fittingly introduced among the sthenic group of cases. I do not know how I came to overlook so well-marked a specimen.

July 25, 1843—Multipara. Easy labour. An attack of pleurisy came on the first week. There was great pain, with dry cough; inability to lie on side affected; lochia and milk checked; pulse 125, sharp. The pleuritic pain was preceded by a general feverish state, which commenced on the second day, and for which I had prescribed Dover's powders in a saline mixture, with vin. ant. tart. Two days after, there was diarrhœa, which was checked by a few doses of chalk mixture, with laudanum and aromatics. The next day, violent pleuritic pain coming on as above described, I bled her freely in the arm, and gave her antimony, digitalis, &c. The following morning, there being little or no relief, I applied a dozen leeches to the side affected; and in the evening, the symptoms being still urgent, I bled her again in the arm, which cut the disease short. I then put on a

blister. She was soon quite well, continuing the antimonial medicine for a few days.

In the reports of cases given in the preceding pages, I have endeavoured to embody in a form likely to be instructive, the whole of my experience of puerperal diseases. In doing that, it will be understood that I have not included the whole of the puerperal diseases I have met with in practice; several of the groups in which I have arranged the facts detailed containing only *specimen* cases, in selecting which I have, I trust, been guided less by desire to parade successful results, than by an impartial wish to insert such cases only as would, in my opinion, best illustrate the various suggestions and opinions founded on them. I have, therefore, not hesitated to insert a large proportion of fatal cases. For the suggestions, remarks, or comments, interspersed among the various groups of cases, or made upon individual facts, I claim no merit of originality; I claim merely this merit—that the opinions expressed were the offspring of much careful and anxious thought on my part, mostly indeed while I was in actual personal attendance on the several cases, and while I was feeling the full weight of the awful responsibility thrown upon me. With regard to the *terque quaterque vexata quæstio* of the treatment of puerperal fever, it is, I think, obvious that the main doctrine of the present paper altogether precludes the suggestion of any one *ready method* of curing puerperal fevers. For while I have endeavoured to show that nearly all puerperal febrile affections are ONE disease, I have also been anxious to point out that the various manifestations of that one disease require varying and shifting methods of treatment. I would submit that the main thing to be borne in mind in these diseases, as indeed in all others, is the distinction between the sthenic and the asthenic varieties of them, a mistake in making this distinction being exceedingly likely to be a fatal one for the patient. I have no *cut-and-dry* plan of treatment to suggest; I would, however, venture to urge that the form so frequently prescribed by me under the designation "*Mistura puerperalis*" (page 144), is an exceedingly valuable one in most of the febrile affections of the puerperal state. To believe with some that *oil of turpentine* will cure all cases of puerperal fever, because it relieves the one symptom, tympanitis; or with others,* that *digitalis* will

* "M. Serres (d'Alais), membre correspondant, monte à la tribune et présente quelques considérations sur la *fièvre puerpérale*.

"Après avoir décrit les accidents généraux qui sont la conséquence de cet

do the same thing, because it diminishes the frequency of the pulse—is not a much more sensible proceeding than that of the farmer who, anxious for a change in the weather, sedulously every morning shakes the barometer.

IV.—*Case of Renal Injury.* By JOHN MURRAY, M.A., M.D.,
Newton-Stewart.

THE deep position occupied by the kidney, and the protection afforded to it by surrounding structures, render this organ much less liable to serious injury than most of the other abdominal viscera. Judging from surgical statistics, indeed, laceration of the kidney, uncomplicated by other injuries of a more immediately fatal character, would appear to be of rare occurrence. The only case I have seen recorded appears in the *Lancet* for 24th June, 1848. The patient was a boy, aged eight years, who was run over by a truck, the wheel of which passed over his loins. The injury was followed by hæmaturia, and at a later period by urgent symptoms of uremia, the specific gravity of the urine having at the same time fallen to 1·005. In the following case, which was recently under my care, the symptoms were of a less severe character, probably from the injury affecting immediately only one kidney, while in the above case both organs appear to have suffered contusion.

J. M'M., aged 30, was a van-driver, of sober habits, and generally enjoyed good though not robust health. Had never suffered from any serious illness except during last winter, when he was laid up for ten weeks by an attack of pneumonia. On the 25th October last he was driving a van, weighing together with its load about a ton, down a hill, when part of the harness giving way he fell upon the horse's back. The animal ran off, and, being unable to retain his position, the man fell upon his left

état, M. Serres indique le remède qui lui a souvent réussi et qui consiste dans l'administration, toutes les quatre heures, d'un granule de digitaline. En quarante-huit heures, tout se calme et rentre dans l'ordre.

"Quant à la question thérapeutique, M. Depaul ne voit pas ce que l'on gagne à agir sur la circulation; car si le sang est infecté, qu'est-ce que cela fait que le cœur batte plus ou moins vite? Le sang n'en reste pas moins en contact avec tout l'organisme; c'est ce qui a été démontré pour le *veratrum viride* et pour le sulfate de quinine. Sous l'influence de ces substances, on a pu obtenir un ralentissement de la circulation tel, que les pulsations tombaient à 40 par minute. Cela n'empêchait pas les femmes de succomber."—*Rapport de la Stance de l'Académie Impériale de Médecine*, 26 Avril, 1859.

side before one of the wheels, which passed over his body in a direction obliquely from upper part of lumbar region posteriorly to crest of ilium. I saw him in about two hours after the accident in a house to which he had been removed in the interval. He was still suffering severely from shock, the extremities being quite cold, and the pulse feeble. Excruciating pain and tenderness on pressure were complained of in right lumbar region, and in this situation slight ecchymoses were found along the course pursued by the wheel. Extending upwards about two inches from the crest of the ilium, was an abrasion, indicating by its character that the wheel had slipped off that bone. No pain on pressure was experienced in any part of the abdomen anteriorly. He had passed urine a few minutes before the accident, and half an hour afterwards about four ounces of fluid, consisting almost entirely of blood, were voluntarily expelled from the bladder. No pain or difficulty was experienced during the effort.

Sinapisms and hot bottles were ordered to the extremities, and sol. mur. morph. \mathfrak{zss} . to be administered every half hour. In two hours, reaction having been established, and the pain being considerably less severe, he was removed to his house in this town, a distance of six miles. He was then cupped, ad \mathfrak{zviij} ., in right lumbar region, and hot fomentations ordered to be applied in this situation. The following mixture was also prescribed:—

R. Vin. antim. tart.	\mathfrak{zj} .
Sol. mur. morph.	\mathfrak{zij} .
Aq. menth. P. ad	\mathfrak{zij} . M.
\mathfrak{zj} . 4tā. q. q. horā.	

26th—Slept well during the night, and when at rest does not feel much pain, but tenderness on pressure is not diminished. Some pain on pressure is complained of in right renal region anteriorly, and also along ureter, but none in supra pubic region. Has some pain and difficulty in micturition; urine very smoky, and contains a large quantity of albumen—specific gravity 1.018; pulse 100, moderately full; bowels have not acted since yesterday. Apply twelve leeches over right kidney, take immediately haust. nigri \mathfrak{zij} , and continue mixture.

27th—Bowels were not moved by the draught, but an enema of salt and warm water procured a copious evacuation. Leech-bites bled freely, and pain and tenderness are much less severe, but the dysuria still continues. Mixture occasions considerable nausea; pulse 84, soft; urine clearer, and albumen less abundant. 24 ounces have been passed since yesterday morning—specific gravity 1.022. Omit sol. mur. morphia from mixture; continue fomentations, and repeat black draught.

28th—Continues to improve so far as pain is concerned, but has only passed 18 ounces of urine, which is more highly albuminous than yesterday—specific gravity 1·022. Bowels freely open; pulse 80.

29th—Had several slight rigors last night. Complains of numbness and formication in lower extremities, but motion and reflex movements are unimpaired. Severe pain on pressure is felt over upper lumbar spines. Pain in right lumbar region is rather less severe, but now some pain is produced by pressure over left kidney. Has almost incessant desire to micturate, but is unable to pass urine until after an effort of about an hour. Has only passed eight ounces of urine during last twenty-four hours—specific gravity 1·022—highly albuminous; pulse 90. Cup over right kidney, ad 3x. Dry cup over left kidney.

30th—Pain in lumbar region was relieved, and no numbness of lower extremities has been felt since cupping. Urine (eighteen ounces) free from blood, but still albuminous; pulse 76, soft. Discontinue antimonial. Pustulate over kidneys with croton oil.

November 3rd—Patient has steadily continued to improve under no other treatment than gentle aperients, counter-irritation by means of croton oil, and nourishing, though non-stimulating, diet. Urine is now normal in quantity and free from albumen, but affords a considerable phosphatic deposit. Its reaction has been acid throughout. From this date convalescence proceeded rapidly. The phosphates disappeared without any special treatment. By the 25th of November he was able to walk about, and on December 1st resumed his work, complaining only of some stiffness in the loins, which was daily diminishing.

In reviewing the chief points of interest in the foregoing case, it will be unnecessary to preface my remarks with an elaborate discussion regarding the general nature of the injury sustained. The mode in which this was inflicted, being in effect by a blow directly over the right kidney, at once indicated that organ as the probable source of hæmorrhage. The absence of symptoms, either immediate or consecutive, of laceration in any part of the urinary passages, and, on the other hand, the unequivocal symptoms of renal irritation which occurred in the course of the case, were of themselves sufficient to establish a complete diagnosis. The precise situation of the renal laceration affords more room for speculation. Whether the friable septa or the Malpighian pyramids were the seat of lesion cannot, of course, in the absence of actual examination, be determined; but this much appears certain, that the solution of continuity extended into the hilum. This, indeed, is what anatomical considerations would lead us to expect; for the secreting tissue, surrounded everywhere else by a fibrous capsule, so resistant that its rupture must almost neces-

sarily be attended with complete destruction of the whole organ, is here only supported by delicate mucous membrane. The hæmorrhage probably proceeded from the rupture of one or more branches of the renal artery after penetrating the parenchyma, where the resistance of the vessels is much diminished.

Though rupture of the kidney, as shown both by the present case and that already quoted, is not necessarily fatal, such an accident to an organ—the constant and efficient performance of whose functions is so essential to life—must in every instance be regarded as of a serious character. Besides the danger of destructive inflammation being excited in the injured kidney itself, there is also the risk, as in idiopathic single nephritis, of a similar affection of the other kidney, in consequence of the increased duty suddenly imposed upon it. The patient in this case was threatened with both results. The diminished quantity of urine, together with rather a low specific gravity, indicated from the first considerable interference with the renal functions, while the dysuria and the tenderness over the right kidney *anteriorly*, where no external contusion existed, showed at an early period that inflammation had been set up in that organ. On the third day the rigors, with further diminution of the urinary secretion, and pain over the left kidney, indicated the second source of danger to be imminent. At this period also the symptoms of spinal congestion occasioned me considerable uneasiness, but fortunately matters soon assumed a more favourable appearance, and the subsequent progress of the case was eminently satisfactory.

The slight hæmaturia, which persisted for several days, and subsequent albuminuria, I was not disposed to regard as unfavourable symptoms. I rather considered them as indicating that the contusion and the consequent inflammation were not so severe as altogether to suspend the secreting power of the injured organ. It is natural to suppose that even if the secreting parenchyma were completely disorganized, the effused blood should find its way along the ureter and mingle with the urine from the unaffected kidney. That this, however, does not occur, at least in all cases, is shown by Mr. Erichsen, who mentions a case in which one of the kidneys was completely smashed and yet no blood appeared in the urine. He appears to consider hæmaturia incompatible with complete disorganization of the kidney.*

Respecting the treatment adopted, it is unnecessary to say much. I chiefly trusted in free local depletion and the depressing influence of antimony on the circulation to avert and subdue

* Science and Art of Surgery. Second edition, p. 321.

inflammatory symptoms. By the diaphoretic influence of the latter also, together with free purgation, I hoped in some degree to avert undue determination to the kidneys. In the latter stages, as already stated, the treatment consisted exclusively in counter-irritation by means of croton oil.

V.—*Two Cases of Concealed Accidental Hæmorrhage at the Full Term of Utero-Gestation.* By J. G. WILSON, M.D., F.R.C.S.E., Fellow of the Faculty of Physicians and Surgeons, Physician to the Glasgow Lying-in Hospital, Member of the Obstetrical Societies of London, Edinburgh, &c.

CONCEALED accidental hæmorrhage, especially when it occurs before delivery, is an accident or complication of a most alarming and very deceptive kind. To the inexperienced practitioner particularly, a case of this description is very apt to prove embarrassing and to throw him off his guard, since the cause of the syncope and other symptoms exhibited by the patient is not immediately apparent—there being no external or visible escape of blood to aid him in his diagnosis. The consequence of this is that much precious time for active treatment is lost, and the safety of the patient more or less endangered before he is aware, and before the real nature of the case is ascertained or recognized. Such a complication is fortunately not one of very frequent occurrence, yet I feel persuaded is oftener met with than the few recorded instances of it would lead one to suppose. The two following cases I consider good illustrations of this form and variety of accidental hæmorrhage.

Case I.—On the 9th of May, 1855, I was called to see Mrs. —, aged 34, a pluripara, of a lax, leuco-phlegmatic habit of body, and who was on the eve of her confinement. This forenoon, when actively employed in the discharge of some household work, she was suddenly seized with great faintness and exhaustion. On the occurrence of these symptoms I was requested to visit her. On my arrival I found her in a state bordering upon complete syncope, with a small thready, tremulous pulse, and general pallor and coldness of the surface of the body. She complained much of nausea and retching, restlessness, vertigo, dimness of vision, ringing sound in the ears, &c. I immediately prescribed brandy and ammonia, which had the effect of reviving her a little. There was no appearance whatever of any external hæmorrhage. The abdominal enlargement was such as is usually met with at the full period of pregnancy. Towards the right side of the fundus uteri there was a distinct, prominent, and

circumscribed swelling, which was not previously observed by the patient. This swelling had a peculiar doughy feel, and was the seat of a peculiar "bursting" sensation. The patient had no labour-pains, and the os uteri was only so far dilated as to admit the tip of the examining finger. Suspecting internal or occult accidental hæmorrhage, I punctured the membranes and administered ergot with the view of inducing labour. In the course of three hours thereafter uterine action supervened, and in four hours subsequently I applied the forceps and delivered the patient of a full-grown but still-born child. The infant presented a very anæmic appearance. During labour very firm external pressure, by means of a compress and bandage, was applied over the abdomen, and particularly towards the fundus uteri. Notwithstanding the free administration of brandy and other stimulants, the patient all this time suffered much from faintness and depression. There was, however, no perceptible increase of the swelling or fulness above mentioned. Immediately after the removal of the placenta two or three very large coagula were passed *per vaginam*. There was no hæmorrhage of any consequence after the discharge of these clots. The placenta, on examination, presented a peculiar bell-shape appearance. It had a large cup-like excavation or concavity on its uterine surface, which was fully occupied by coagulated blood. This hollowed out appearance was evidently occasioned by the pressure of the effused blood. The placental substance, towards the centre especially, appeared much denser than usual—this was also ascribed to the compression exerted by the extravasated blood. The placenta seemed free from all disease. The patient made a slow recovery, but eventually did well.

Remarks.—In this case I think that the sudden and extreme exhaustion was, beyond all doubt, due to occult intra-uterine hæmorrhage. The appearance of the placenta was such as to warrant the supposition that the hæmorrhage was in a great measure, if not altogether, confined within the limits of the placental site. The fulness observed, and the "bursting" sensation experienced on the right side of the uterine fundus, was evidently caused by the circumscribed effusion of blood in the utero-placental region. From the pale and exsanguine aspect of the infant, I am satisfied that its death was occasioned by the hæmorrhage. To no other cause could I attribute its death. The same plan of treatment should, in my opinion, be had recourse to in cases of this kind as would be adopted were the blood making its appearance outwardly.

The following is a rare instance of fatal hæmorrhage, altogether confined between the placenta and uterus.

Case II.—In September, 1853, along with the late Dr. Alexander Stewart, I inspected the body of a woman who died suddenly undelivered, apparently at the full period of pregnancy. On inquiry we ascertained that she was engaged with her ordinary domestic duties, in her usual state of health, till within two hours of her decease. Sudden and alarming faintness was the first symptom that attracted the notice of her friends, and she is reported to have been “just out of one faint and in to another” till she died. No blood had escaped externally. The abdomen generally presented the usual appearance observed at the termination of the ninth month of gestation. On the left side of the fundus uteri, however, a prominent but irregular fulness or swelling was observed, which had a soft doughy feel, and emitted a dull sound on percussion. On opening the uterus a full-grown female infant was found with the head presenting. The child had a blanched exsanguine appearance. The membranes were unruptured. The liquor amnii was quite untinged by blood. The swelling above referred to was found to correspond exactly with the situation of the placenta. Except at its extreme circumference, the placenta was wholly detached from the uterus, and the intervening cavity was distended by a very large quantity of blood in a partially coagulated state. The placenta was of the average size, and exhibited no appearance of disease. Careful examination of the other abdominal and thoracic viscera revealed nothing abnormal.

Remarks.—This case is interesting not only on account of the concealed character of the hæmorrhage, but also in consequence of its confined situation, and the unfortunate result to which it led. It clearly shows and fully proves what I know is disbelieved by some, and very much doubted by many, viz., that a quantity of blood, sufficiently copious to destroy maternal life, may be effused between the uterine surface of the placenta and the corresponding uterine wall. The placenta in this case, as in the last, presented a peculiar bell-shape, or basin-like appearance; with the exception of its margin, it was entirely separated from its connection with the uterus, and the interspace between the two contained an immense quantity of coagulated blood. This detachment of the placenta was apparently of spontaneous origin, there being no evidence of external violence. It is certainly remarkable that the edge or circumference of the placenta should have maintained so perfect and complete adhesion to the uterine parietes, while the central portion was subjected to so much stretching and distension. Two cases somewhat similar to the one now related are mentioned by Drs. Hardy and M'Lintock, in their “Report on Midwifery and Puerperal Diseases” (p. 194). An instance of the same kind is recorded in the *New*

Medical and Physical Journal (1813, No. 38, p. 535). The post-mortem examination in this case showed—"that a separation of the centre of the placenta from the parietes of the uterus had taken place, whilst its edges were completely adherent, forming a kind of cul-de-sac, into which blood had been poured to the amount of a pint and a half, which had become coagulated within the cavity thus formed."

The subject of concealed uterine hæmorrhage appears to me not to have received from writers on systematic midwifery that amount of attention and consideration which its importance demands. In many obstetric treatises no allusion or reference whatever is made to such an occurrence. The *Glasgow Medical Journal* for April, 1854, contains a very excellent paper on this subject by Dr. Joseph Bell of this city. Dr. Bell illustrates his paper by the details of three most interesting cases. In the *Transactions of the London Obstetrical Society* for 1860 will be found a valuable communication on the same important subject by Dr. Hicks, of Guy's Hospital, in which he relates twenty-three instructive cases, collected from various sources. Both of these papers will amply repay careful and attentive perusal.

VI.—*The Social Position of the Profession.*

THE members of the Medical Profession are a long-suffering class. They remain passive and quiescent under neglect, indifference, or open ill-usage. For many a weary year did the assistant-surgeons of the navy resign themselves to the degrading position which they occupied. The poor-law surgeons of England, are even yet but half alive to the miserable state of servitude in which they exist, and but poorly support Mr. Griffin in his determined efforts to ameliorate their condition; while the army staff appear afraid to move actively, in the endeavour to maintain the privileges so recently accorded them, and now partially withdrawn. But the case of private practitioners is in many respects as bad, if not worse, than that of men engaged in the public service. The latter can join together so as to have something like united action; the former, either fight singly in a feeble and losing struggle, or surrender themselves ignobly to circumstances which, they say, they cannot control or alter. Probably it is because of the wide-spread dissatisfaction that we get the character of being a pugnacious class among ourselves. All the spleen and venom engendered by disappointment and chagrin find vent in intestine jealousies and disputes. Whether this be

correct or not, there can be little question that, if the medical temper be choleric, it is sorely tried. No men meet with such scant acknowledgments of benefits conferred, none work more for nothing, and no profession or calling is more inadequately remunerated.

Our object in this paper is to call attention to the social position of medical men—not with the mere view of gratifying discontent, but rather having mainly before us the suggestion of means by which it may be improved, and strongly urging the adoption of these means upon our readers.

A. Of course the social position of the profession differs, both as regards individuals and localities. But it may be assumed as a general rule, that medical men stand on a higher platform, on a better vantage-ground, the higher and better educated and more liberal the community in which they labour. Thus, it is unquestionable that in the great commercial cities of the three kingdoms the physician and surgeon are less appreciated than they are in the capitals, or in the smaller towns, where cotton or money do not reign supreme. In the former the practitioner is looked upon with a purely mercantile eye—a man whose visit requires to be debited in the ledger; whose account is to be docketed and paid at Christmas, like that of the tailor; and whose debt is fully discharged by a certain amount of the current coin of the realm. But little sympathy is felt for him as a labourer in a noble field where science and humanity go hand in hand; and there is but slight appreciation of the weight of all those mental anxieties which must ever bear heavily on the trustworthy and honest. If we reflect for a moment on the way in which so often the earnest gratitude expressed in times of physical prostration evaporates with returning strength, or perhaps is supplanted by indignation at what seems a heavy bill—the truth of the above remarks will be at once acknowledged. Is there one of us who cannot recall instances in which protestations, made in the times of darkness when our presence was hailed as that of a guardian angel, were supplanted by the coldest indifference when health was restored.

There must be much of this also even in what are deemed the great professional centres; but there it is redeemed by the higher social position occupied by the practitioner. Mingling with representatives of other learned professions, men capable of estimating the dignity of our art, and who know that it can only be mastered by good intellects and unwearied study—the doctor takes his proper place in society, is valued not only for his practical skill, but for his scientific attainments and his wide-spread philanthropy. There, he is the equal and the friend, as well as the willing servant.

To make our case perfectly clear and plain, we will select this good city of Glasgow with its four hundred thousand inhabitants; and we ask our readers, most of whom will be able if they please to answer the question—Does the medical profession possess that influence which it ought to have, and which it has elsewhere? do its members hold a prominent position among the citizens? are they consulted on those general matters in which their experience, education, and talents qualify them for giving sound advice? do they, in fine, occupy that social position to which they are entitled, if ability, and learning, and public usefulness count for anything? We presume no one of us can honestly answer in the affirmative. We are not asked what sanitary enactments are required; our services are not demanded to enlighten the lieges on the means of preserving health; our mouths are peremptorily shut if we venture to offer opinions as to the construction or site of hospitals. Any layman who busies himself about such matters will prove himself a match for the profession and carry his position, however false or untenable it may be.

Writing in a professional journal, it is unnecessary to enter into details. Our readers will, we are confident, recognize the truth of the fact, appearing though it may in different aspects to different minds, that here, the social status of the profession is not what it ought to be, but immeasurably below the proper standard. Probably, the same is true of the legal profession, at least to some extent. The only learned body of any weight in the councils of Glasgow is the clerical—a fact which is easily accounted for on Mr. Buckle's theory.

B. Unfortunately there is no sovereign remedy for the evils we deplore, but there are several means by which we are sanguine a better state of things could be attained. And first of all, where money reigns supreme, there is the monetary remedy—a most effective method if we had *all* the courage to employ it. In that little word *all*, however, lies the force and the obstacle. A combined determination on the part of the profession to obtain adequate remuneration for their services, effectively and persistently carried out, would do more to raise the status of the profession than any other means. To procure any consideration in a rich community, we must be, if not rich, at least free from the pressure of narrow circumstances. We must be in a position to aid in public charities and public movements, to act independently, and to secure a moderate reversion for those who are dependent on us. Now, what is the ordinary result of a professional man's career in Glasgow? Ten years of waiting inaction, or of miserable servitude behind a counter, followed by, say, twenty or thirty years of badly-remunerated toil and

worry, to end either in an old age with restricted comforts, or in leaving those who have been accustomed to better things, to struggle through their race on a wretched pittance. Supposing a practitioner to be busily engaged every day of his life, and paid at the usual rate, it would hardly be possible for him to keep up the position of a gentleman, and at the same time suitably educate and provide for his family. But we are not always busy, and even when we are, how much of our work is there for which there is no pecuniary return! Further, another element has of late aggravated our position, and that is, the inroad made on professional incomes by the increased summer migration which has followed on the greatly extended facilities for travelling. It is all right and proper that the public should enjoy themselves, but as the merchants feel the scarcity of cotton, and enhance the prices of grey cloth, so must we, suffering from an enforced idleness of six months, raise our demands for the other six. We say unhesitatingly, that we never knew of a practitioner in Glasgow, who had no public appointment and no private means, who was able to leave his family in anything like affluent circumstances; and there are few, even of those who have held appointments, who have been able to do so.

One remedy then is, clearly, to raise our fees, undeterred by fear of losing our patients. In truth, the dearer advice is, the more it will be valued, and it would be infinitely better to make three visits daily at ten shillings each, than twelve at half a crown—better for the doctor, and assuredly better for the patient. It would be a curious arithmetical problem to determine how it can remunerate a physician living west of Buchanan Street, to pay a visit in his carriage and pair, say in Eglinton Street, and receive half a crown. Let us further put the matter to the test of figures. Taking an extreme case, say a physician could make twenty visits per day, and suppose he was paid five shillings for each, we have a weekly return (excluding Sunday) of £30, which makes an annual income of £1560, without allowing for a single day of illness or relaxation. But we all know that few, if any, ever make such a number of visits at such a remuneration; and we firmly believe that the number of medical men who clear from practice more than £500 or £700 a year, little exceeds the digital numbers. Of course there are extra fees for midwifery, operations, &c. &c., but the gross incomes of £1000 a year in a town where house rent, taxes, poor rates, and the other ordinary domestic expenses are unusually high, and where there is a vast accumulation of wealth, are so few, that none of us would like publicly to admit the facts as they stand. It is easy by reducing the fees to half a crown, or the number of visits to one half, to see what probably is the actual condition even of those who

are considered most successful. Contrast this with the colossal fortunes amassed around us. Surely the talents and the skill which contend with disease and death have the same right to just remuneration as those which are devoted to the manufacture of iron, or the sale of handkerchiefs?

Again, we stand in our own light in other ways. We are too apt to give gratuitous advice to those who are able to pay, from a false delicacy in not asking when we are not offered a fee; and it seems with some to be the custom not to charge for house advice at the same rate as for visits. Of late, too, agreements have been entered into, we should think, without due consideration, by which the actual attendant only receives a half fee. This is most suicidal policy, for it is admitting that such a fee is remunerative. That cannot be looked upon as a charitable act to individuals, which is so decidedly injurious to a profession.

Next to the pecuniary evil, on which we might expatiate at great length if we said all we felt on the subject, comes the want of a proper and dignified independence, which, indeed, is partially a result of the others. The doctor is too apt to allow himself to forget his position and office, and to place himself at the beck or call of any one who may send for him, no matter the time or circumstances. He is apt (naturally enough, God knows, when he is hard pushed for a subsistence), to be too indulgent to the whims and vagaries of his patients; and on the whole, to be too amiable. We would not have him assume the surliness of Abernethy, or ever forget the gentleman in the doctor, but there is a limit, and he should always remember the dignity of his profession, and maintain his position as an educated man. He should not unnecessarily undertake the functions of a nurse. Nor should he be afraid to express his opinions on political or on ecclesiastical matters. From the fear of giving offence to his patients, he is apt to be silent or acquiescent when such topics are discussed. There can be no greater weakness, and none more fatal. The result is contempt. Of all men, physicians should, supposing they have any, be bold in enunciating their opinions. The doctor too should take an active part in any public movement in which he is interested, or on which he is well informed, not with any selfish view, still less from any inherent passion for platform notoriety, but as a debt due to his profession, and with the object of upholding a character for public usefulness. On all occasions he should exact the precedence and deference due to him by the usages and laws of good society. In short, as a class, we should ever bear in mind that we are representatives—the representatives for a few years merely—of a profession sacred in its character and calling, which has conferred the most inestimable benefits on mankind, which

has transmitted to us great privileges, and these privileges we are bound to transmit to our successors, not diminished, but preserved and augmented. Any act on our part, or any omission which tends to lower our status one iota, makes us traitors to our craft.

The ventilation of this delicate subject can do no harm, and may result in good. We cannot pretend to do more than introduce it to the notice of the profession, in the hope that in other hands it may be further and better elucidated and exemplified. But most certainly the main onus of correcting the anomaly of an underpaid medical profession in a city possessing the wealth of Glasgow, lies on the seniors of the profession. Let the *upper ten* do their duty, at once fearlessly refuse inadequate fees, and they may reckon on the generous support of the rest.

VII.—*On the Treatment of Chronic Swelling of the Bursa Patellæ, by Puncture and the Injection of a Solution of Iodine.* By ROBERT KIRKWOOD, M.D., Larga.

THE ordinary methods of treating chronic swelling of the bursa patellæ by counter-irritants, &c., being troublesome, painful, and tedious, and the result unfortunately after all seldom satisfactory, I resolved, in a case which came under my care in 1859, to try the effect of puncturing the tumour, evacuating its contents, and injecting a solution of iodine. I hoped, by altering or destroying the secreting surface of the tumour, to effect a speedy and radical cure, and my anticipations were fully realized by the complete success of the operation. I have since treated two other cases by the same method, and the results having been equally successful, I am induced to bring them briefly under the notice of the profession.

My first case was that of a young woman, a domestic servant, who more than a year before had observed a small, painless swelling, for which she could assign no cause, in front of her right knee. It increased slowly in size, and when I first saw it, was about as large as an orange, and soft and fluctuant. She complained only of the weakness and inconvenience which it occasioned her. With a trocar and canula, such as is used in the operation for hydrocele, I punctured the swelling at the most dependent point, and after evacuating the straw-coloured serous contents, injected a solution of iodine and retained it for a minute or two. The canula being now withdrawn, a piece of lint was placed over the wound, and a short straight splint applied along

the limb and retained with a roller. The following day patient complained of a little pain in the knee, and there was also slight swelling, which was fluctuant. I passed a probe along the course of the puncture, and evacuated a small quantity of a brownish-coloured serous fluid. There was no constitutional disturbance; the local was so trifling as scarcely to deserve mention; and by the fifth or sixth day, I forget which, the knee, though a little weak, was perfectly well.

The second case, also occurring in a young domestic servant, bore considerable resemblance to the first, with the exception that from its history it appeared to be the consequence of an attack of acute bursitis; and besides the weakness and inconvenience to which it gave rise, was at times also more or less painful. It was treated in precisely the same way and with the same satisfactory result.

The third case occurred in a young man, a joiner, and was more recent than the others. The stiff, firm, leathery feel of the tumour gave the idea of a thickened sac, and this, after puncture, was found to be the case. The contents were sero-sanguineous. On two different occasions I passed a probe along the course of the puncture to evacuate a small quantity of fluid; but in five days the knee was well, though by my advice patient did not resume his work till the expiry of a week from the date of the operation.

It is now about three months since I operated on this case, and the cure up to this date seems perfect. Some thickening still remains, but under the use of iodine paint it has diminished considerably. The second case was operated upon in June of last year. The patient has gone from under my observation, but her knee, her mother informs me, is strong and well, and shows no indications of anything having ever been the matter with it. I saw my first case the other day; no trace remains, and patient states that her knee is as strong as ever.

In each case, in consequence of a slight feeling of weakness remaining after the operation, though less than before it, I recommended an elastic knee-cap to be worn for a few weeks, by the end of which time the feebleness had disappeared. The splint I employ simply as a precautionary measure, with the view, by limiting the use of the limb and the motion of the knee, of obviating or lessening the risk of severe local action.

The results obtained in these three consecutive cases establish, in so far as such a limited number can, the superiority of this method of treatment. By an easy and almost painless operation, and without, so far as my experience goes, any drawback whatever, a troublesome and often intractable affection may be safely and permanently cured in a few days.

VI.—*Anomalous Case of Convulsions.*

By CHARLES F. MACLACHLAN, M.D., Rothesay.

ON the night of the 24th of September last I was called into the country to see J. G., who, it was stated, had been seized with nervous fits during the evening. On my arrival I found him to be a stout plethoric lad about 20 years of age. He complained of violent headache; his pulse was 90, full and hard, and he had very frequent convulsive fits, confined chiefly to the right side of the face, neck, and body.

I inquired into the history of the case, and discovered that ten years ago he had a similar attack which lasted nearly three weeks, during which time he underwent a variety of treatment; seven years later he had a slighter attack; but after both seizures a slight paralysis of the organs of speech remained for some time. His father told me that when in health he was an active hard-working lad, and that he had been busily engaged taking in the harvest up to the time he was seized. I was also told that he had been smoking a great deal of late, an indulgence he was not much accustomed to. His bowels were generally regular when in health, nor was any evidence of the presence of worms discoverable.

Nature of the Convulsion.—The patient said that the fit usually began with a feeling of numbness in the fingers, which spread up the limb; the arm then became convulsed; the spasm next seized the throat, when a peculiar crowing sound was emitted in short jerks as it were; the right side of the face became rigid to the touch, but along with the eyelids was in constant motion; there was decided strabismus, and the mouth was drawn to the left side; this generally continued for some time, when the fit ended with foaming at the mouth. During the severest fits when I was present, the pulse remained unaffected; and in one of them, before I saw him, he had bitten his tongue severely. The curious phenomenon was this, that in the paroxysm he retained his consciousness, but was unable to speak, and he continued conscious in the intervals. I ascertained that he was conscious during the fit, in addition to his own statements to that effect, by pinching his left arm, when it was always drawn hurriedly away.

Treatment.—As he was of full habit, with a hard full pulse, injected eyes, and violent headache, the treatment was from the first antiphlogistic. V.S. was employed twice, and on both occasions with marked benefit; he was freely purged with calomel, haust. nigr., and croton oil, but still the paroxysms, though much diminished in violence, recurred very frequently, and were now

more confined to the face. I discovered that even when in repose his mouth was twisted to the left side, and when asked to put out his tongue it was protruded to the right; his speech also became thick, and he had evident difficulty in articulating. Along with these symptoms he complained of a choking sensation in the throat, like a ball which seemed to rise from the stomach, before the fit came on; and I now noticed that during the intervals when spoken to about his disease he cried very readily.

He was now far less flushed, and the pulse had fallen to 70 and was soft, while the headache was almost entirely gone. However, I thought it prudent to cup him on the nape of the neck; his head was shaved and blistered, and pills of oxide of zinc and hyoscyamus were prescribed for him. These measures seemed to mitigate the disease very considerably; but the convulsions still continued, though in a much more chronic form. I now put a seton in the neck and ordered him pills of the valerianate of zinc, and from that time he improved daily. From beginning to end the convulsions continued for sixteen days, and during that time recurred at very short intervals.

This case is a very peculiar one, and appears to me to be of a mixed character; for there are some features in it resembling epilepsy, and others again of a quasi hysterical nature. In addition, there is the partial paralysis of the mouth, tongue, and other organs of speech, indicating some irritation or lesion of that portion of the nervous centre from which they are supplied with nerves. I could discover no eccentric cause for these fits, and am therefore inclined to think them centric in their origin, and to be the result of some local congestion of the medula oblongata and upper part of the spinal cord. In this way, too, the entire absence of insensibility may be accounted for.

I have not seen the patient lately, but have been told that he has never had a recurrence of the fits, though some slight paralysis of the organs of speech still remains.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

I.—RECENT WORKS ON SKIN DISEASES.

1. *On the Parasitic Affections of the Skin.* By T. M'CALL ANDERSON, M.D. London: John Churchill, 1861.
2. *A Guide to the treatment of Diseases of the Skin: with suggestions for their prevention.* By THOMAS HUNT, F.R.C.S. London: T. Richards, 1861.
3. *Ueber Syccosis und ihre Beziehungen zur Mycosis Tonsurans.* Von H. KÖBNER in Breslau. Virchow's Archiv, 22^{sten} Bandes 3^{tes} und 4^{tes} Heft. Berlin, 1861.

A WRITER on skin diseases at the present day, whatever the character of his work, would be unwise to expect for it immediate popularity. The profession is naturally distrustful in cases where it has often been deceived. Books on skin diseases are too often little more than legitimate media for advertising, containing nothing that is new, and presenting nothing in a better form than that in which it is already familiar. But a good work survives distrust, and two of the titles at the head of this article belong to contributions of sterling value. Dr. Anderson's work is the production of a practical physician, who, having detected a want in the literature of his profession in this country, and consequently in the presumable knowledge of his countrymen, has recorded for their benefit his own judgments, which have been formed in a wide field, with great opportunities for observation, and great good sense in taking advantage of them. Herr Köbner's is an article by a man of science enthusiastic in his vocation, who, after laboriously collecting materials to illustrate a particular disease, has drawn such a picture of it as has never before been given, and which will not readily perish from the literature of dermatology.

Mr. Hunt's work is the most general of the three; the reviewer would have rejoiced could he have applied to it similar commendation to that pronounced on the other two. Mr. Hunt has acquired an almost European reputation for the following original notions. A skin disease, the general health being attended to, "will either get well spontaneously or not. If not, it is either syphilitic or otherwise." A syphilitic disease is to be "treated accordingly," i.e. with mercury; an "otherwise" disease is to be treated with arsenic. This is a classification of skin diseases and system of therapeutics of admirable simplicity, which Professor

Hebra is in the habit of facetiously taking notice of in his lectures. It must be allowed that the author's remarks on the management of an arsenical course are judicious and even valuable. But arsenic is to be administered not only in constitutional inflammations, it is recommended for ringworm and pityriasis versicolor, as well as for nævus and warts. Still there are good practical suggestions in this book; take, for example, the chapter on ulcers, the excellent enforcement of the benefits of early rising, and the chapter that has just been added on the uses of the Turkish bath. On the whole, however, it is a dangerous "Guide to Treatment," and the advances of science now-a-days are too rapid to leave many so uninformed as to believe it theoretically sound. It would be foreign to the scope of this review to follow the author, who is an avowed Willanist, through a work with many shrewd observations, but of which it must be confessed that it does not excel, even at the present day, so much as it falls short of Bateman's treatise. It must suffice at present to notice the views entertained on parasitical affections.

Mr. Hunt's complacent reference to his experience of "more than thirty years," and his connection with a metropolitan dispensary, ought, within certain limits, to command for him respectful attention. The conservative tendencies of his mind may make him contemplate with excusable regret the traditions of his early days expiring round him; but, when a new doctrine gains general acceptance among scientific men, a man must inevitably compromise his reputation if, without better grounds than Mr. Hunt has to give, he obstinately refuses to believe it. Such a doctrine is that of Mr. Hunt's "parasitic diseases, *if such there be*;" utter disbelief in which can only be accounted for by a resolution to misinterpret the evidence of the microscope, and to set against the unimpeachable testimony of well-known observers, the vague conclusions of experimenters of no authority. The author says "scabies is caused by dirt;" that "a clean young man may take it by going into dirty company;" and that "the acarus is an accidental result of dirt!" He denies the conclusiveness of the experiments of M. Albin Gras, which are cited in Mr. Wilson's book, in which the introduction of acari on the skin of healthy persons was followed by the formation of their characteristic furrows with itching and vesicular eruption. He shows his real ignorance of the subject, by maintaining that the acarus is never to be detected in a vesicle; whereas any observer of ordinary enthusiasm must have detected it in many of these, although the greater number of vesicles, as of the other pathological lesions, are the direct results, not of the insect, but of the scratching induced by pruritus. Were a traveller in the East, passing through green and luxuriant meadow lands, to encounter one of

those flights of locusts that sometimes darken the air for miles ; were he to note that on the fields on which they had settled the green luxuriance was fast disappearing, and that the tracts over which they had swept in their course were barren of all vegetation ; and were he then to conclude that vast districts of country, on which the locusts had been feeding, were merely unproductive owing to some salt in the soil, and that the locusts were merely present as an accident of the salt, it would not be a whit more opposed to common sense than are the views entertained by this imaginative dermatologist. It might be argued that the acari are in reality less numerous than the locusts in proportion to the symptoms of their presence ; but then the most of these appearances are caused by scratching, and not by the parasite. It is different with vegetable parasites, as those of favus and ringworm, the spores of which infiltrate the hairs and epidermis in countless myriads in every case. Mr. Hunt quotes at length "Mr. Jabez Hogg, the eminent microscopist ;" it would perhaps have been as well had he been a little more distrustful of this eminent man. Mr. Hogg declares, *inter alia*, "that the fungi supposed to be peculiar to certain diseases of the skin are also to be found in many other diseases of the cutaneous surface," which is simply not true ; an assertion which any competent person, investigating the matter for himself with the microscope, will speedily find out to be the case. He declares "that attempts have been made in vain to implant these parasites in a healthy skin ;" an assertion this time perfectly correct, only that he omits to add that *other* attempts have been made successfully by Remak, Bennett, Deffis, and many others. He concludes "that special parasites, peculiar to and productive of special diseases, *do not exist*." It was hardly necessary for Mr. Hunt to have added "that Mr. Hogg's views have been impugned by one or two correspondents of the medical journals ;" of course they were ; and it would be well for the author to adhere to his resolution of "not presuming for one moment to offer any opinion on the microscopical bearings of the question." As to his dogmatic assertion "that the alleged parasitic affections are *never* cured by destroying the parasite, it is simply to be met by opposing authority to authority ; that of the greater number, the most scientific and most eminent of living dermatologists on the one hand, to that on the other hand of Mr. Hunt and Mr. Hogg.

Dr. Anderson seems to have studied the parasitic affections of the skin much more profoundly and to greater purpose than Mr. Hunt. The symptoms of favus and ringworm affecting the scalp are better described than in any other English work ; while the views on treatment are mainly those of the modern Parisian school, which deserve more attention on this side of the channel

than they have hitherto obtained. The end proposed by them is the destruction of the parasite in the hair follicle; the mode of arriving at it is not simply by depilation, which would not be new, but by a systematic and careful depilation, hair by hair, both of the parts obviously affected, and also of those in the neighbourhood, to the hairs of which the morbid vegetation may presumably extend. In spite of Mr. Hunt's private opinions, there can be no doubt of the public fact that numerous and lasting cures are the result of this method of treatment; provided always that it be faithfully and perseveringly carried out. But there is equally no doubt that numerous failures are on record, due to carelessness or premature abandonment of a process which, it must be admitted, is very troublesome, and only not tedious when compared with rival proposals.

The only subdivisions of *favus* of any consequence are those which refer to the structures attacked by the disease. Dr. Anderson thus notices *favus pilaris*, *epidermidis*, and *unguium*. To *favus* of the epidermis he assigns a secondary rank, meaning perhaps exclusively *favus* of the smooth skin; whereas *favus epidermidis* is perhaps invariably, and in all situations, the first stage of the disease. Parasitic spores infiltrate the epidermis of the scalp before they descend into follicles; and spores in the epidermis of the extremities attack the downy hairs, as in the scalp. Now Dr. Anderson notices the development of *favus pilaris* on the smooth skin; it were to be wished that, instead of merely mentioning the epidermic form, he had selected it, with its gradual complication with *favus pilaris*, as the most simple and beautiful example, not obscured by any covering, to illustrate how the parasite is sown and grows, how it spreads to the hairs as a *favus pilaris*, and how, on destroying it, the *favus* is cured. The preliminary symptoms, however, on the head are well described, although their *rationale* from spores vegetating in the epidermis is apparently left by the author to be inferred; if his questionable statement about the parasite *falling into* the hair follicles does not point to a somewhat different interpretation of the preliminary erythema from the true one.

"The disease generally commences with itching, followed by the appearance of the cup-shaped crusts; but in many cases the pruritus is accompanied by an erythematous state of the skin. This redness, accompanied by slight swelling and desquamation, may be diffused or circumscribed. . . . This erythematous condition is more frequently seen when the disease extends to the body; but it is probably just as frequent on the scalp, where it is overlooked, from being hid by the hairs, or the disease has passed this stage before the patients find themselves obliged to seek advice. The yellow crusts soon appear in the place of the erythema, or we may see the red patches at one spot and the cups at another."

Dr. Anderson's explanation of the so-called *predisposition* to

favus is simply that the fungus flourishes better on a rich soil than on a poor, on a dirty skin than on a clean one. This is surely much more natural than the theory of Mr. Hunt, who, having persuaded himself, it is to be supposed on the authority of Mr. Hogg, that the achorion crusts are nothing but "dried yeast," proceeds to account for this vegetating in *porrigo lupinosa*, by assuming "that blood may be reduced to so low a scale of vitality (!) as to form a nutrient soil for the parasite." Mr. Hunt also denies the contagion of favus, and regards it as in this country a very rare disease.

While Dr. Anderson speaks of epilation with favour, as practised by means of the forceps, he does not seem much inclined to approve the employment of the *calotte*. The application of the calotte consists in spreading strips of a firmly-adhesive plaster on the scalp, with the hairs closely cut; these are presently torn off, not suddenly and forcibly as Dr. Anderson suggests, but with care, and bring along with them nearly all the hairs adherent by their free extremities. The appearance of the hair roots and sheaths, standing thickly set and erect upon one of these strips, is somewhat peculiar, and reminds one of the back of a porcupine. The most strongly urged objection to this process has always been its cruelty; which, now that chloroform renders the avulsion painless, is of little force. It ought, however, not to be regarded as a *substitute* for regular depilation, because it is not so thorough in its results; but in many cases of widely-spread favus, where the hair of the whole head must be removed, it should not be rejected, since it shortens the process to use it first, the hairs that have escaped being afterwards removed in the ordinary way by the forceps. The calotte is much employed at Lyons and in many parts of France, as also with good success at Berlin in the hands of v. Baerensprung.

After the treatment of favus, Dr. Anderson describes *tinea tonsurans* or ringworm, which he subdivides with Hardy (for the nomenclature followed is less strictly that of Bazin, as averred, than of his colleague) into *herpes circinatus*, *herpes tonsurans*, and *sycosis*. Ringworm of the scalp and of the body are discussed with much the same ability as favus, and from the same practical point of view. The mention of sycosis brings under consideration Herr Köbner's article in Virchow's Archives, which must be acknowledged as by far the most perfect that has yet appeared on the subject; while Dr. Anderson's practical exposition brings before the public in an English dress the greater number of important facts in the history of this controverted malady. The truth is, that *sycosis parasitica*, Dr. Anderson's *sycosis proper*, is a perfectly different affection from *non-parasitic sycosis*, which is a peculiar form of acne limited to hairy parts (*acne mentagra*,

Hebra; acne sycosiformis). This distinction is enunciated by Köbner in the title he prefixes—"On sycosis, and its relations to mycosis tonsurans," i.e., to its parasitic form. Now it happens that, in Scotland at any rate, *sycosis parasitica* is very rarely to be seen; probably because, as Dr. Anderson suggests, men are more addicted to shaving with their own hands, and less to saluting their neighbours in the southern style with a kiss. On the other hand, *impetigo pilularis* and *acne sycosiformis* are very common diseases, and both are often vaguely thrown together as *sycosis*; a title to which the acne, although not the impetigo, has a perfect and long-established right. But, once more, it cannot be too often insisted on that *sycosis parasitica* and the usual tubercular *sycosis* have nothing in common except their seat and their tubercles. The common *sycosis* of the country is a species of acne; the parasitic *sycosis* is ringworm of the beard; while so much do these conditions seem to be reversed abroad that, according to Köbner, 63 to 69 per cent. of the *sycoses* occurring at the St. Louis are parasitic. It would be out of place to enter on the distinctive diagnosis of the affections here; its principles are well understood, and are clearly laid down with scarcely any difference in both the treatises under review. Ninety cases in a hundred of the parasitic form are to be referred, according to Köbner's statistics, to the use of foul razors, which transmit from person to person the spores of the parasite; and from 3 to 5 days after a *foul shave*, the eruption, at first a pure *herpes circinatus*, makes its appearance on the chin. The translation of a portion of Köbner's description of the first symptoms observed may not be unacceptable to the readers of this review.

"Patients attacked by this form of *mentagra*, observe for from 6 weeks to 5 months, and even longer, on the cheeks, lips, chin, anterior surface, or more seldom the nape of the neck, a circle, erythematous over its whole surface, or with the redness only indicated at the imperceptibly-elevated border. The circle sometimes itches, sometimes not; sometimes there are a few small miliary vesicles at its margin which rapidly dry up; sometimes none at all. The erythematous circle, or circles—for several often appear simultaneously—enlarge peripherically in a uniform manner, or only in particular directions in a zig-zag form. On these, after a few weeks, appear numerous *mealy spots*, upon which the hairs are deficient in a manner the more obvious the later the process occurs. The portions of the skin thus attacked are not, however, bald, but are covered over with very short hairs nipped, as it were, in their growth. The shortest hairs are those that occupy the centres of the circles, which protrude in the form of short points, or, should a group be simultaneously attacked, in the form of little brushes. The remaining hairs of the circles remain as long as before, and without breaking; only that the most of them exhibit a (parasitic) covering of the shaft that may be likened to hoar frost. Matters remain in this condition for some weeks or months, the changes going on in a gradual manner, and in a way that occasions almost no inconvenience to the patient. He may remain at least eighteen months, so far as my own observations go, with such an eruption over the hyoid bone, or on the cheeks. But, in by far the greater number of cases, even during

the first month, red papules the size of a lentil, and in other cases little pustules are formed round some of the diseased hairs. Owing to gradual enlargement, increase in number, and confluence with one another, the pustules are ultimately transformed into thin crusts, and the papules into tubercles of a livid red, of the size of a bean, a cherry, and in well-marked cases, even of a hazel-nut. Small pustules form by degrees on the surface of these tubercles, which presently giving way, and their contents drying up, the tissue beneath them continues to secrete a minute quantity of thin pus. A few weeks after the commencement of suppuration the hairs on the tubercles fall partially out; others are involuntarily removed by the patient as he picks the crusts away; but the greater number of them remain broken short. . . . It is in the first instance owing to external applications, which the most careless patients no longer refrain from using for the deformity of their faces, that the tubercles cease to remain isolated on a surface of skin which, although much injected, scaly and excoriated, is still almost level. But there now extends from the tubercles as centres a uniform infiltration of the corium and cellular tissue round about, from which these structures may swell to twice or thrice their normal size, and become hard, shining, and hot. Meanwhile the sharply-defined circular rings become effaced at their periphery, so that only little segments are to be met with; the scales are for the most part removed, or obliterated by purulent discharge," &c.

The following remarks are of great interest, as illustrating the manner in which the spores of the parasite make their way into the interior of the hairs :—

"Let one examine the lower fragment of a hair springing from a tubercle, and that has been left behind in its follicle. It will be found filled with spores of the parasite, usually from the oblique end, at which it has broken off downwards to the bulb. The spores will be most numerous in the lax and swollen bulb, from which point they have evidently pushed up into the hair. In other cases, however, they do not extend to the bulb, just as sometimes the spores of the sheath do not extend to the undermost part of the follicle. In these cases they must have introduced themselves into both fragments of the hair after its rupture in a sheath that was full of them; and must, therefore, have pushed themselves into the lower fragment from the point of rupture, and towards the bulb. One, nevertheless, observes regular *tonsures* much seldomer in the beard than might be expected from their constancy on the head, because the advanced alterations of the hairs that have been described only affect a certain number, between which there are others more free from the parasite, or that have even escaped entirely. The infiltration of the hairs in their sheaths as a consequence of previous rupture, which is very common in the head, and still more so in downy hairs, has rarely been detected by me in the hairs of the beard, without doubt, on account of their original consistency."

Herr Köbner illustrates his article by the citation of nine highly instructive cases; in one of which a father was affected with sycosis, his eldest son who slept with him with herpes tonsurans of the scalp; his wife was affected with herpes circinatus of the upper lip, and a three-year-old child, who slept with her, with herpes circinatus on the neck. His description of the parasite is so complete in every part as to leave scarcely any room for a single addition, and is illustrated besides with excellent drawings in two plates at the end of the number.

Alopecia areata, otherwise *porrigo decalvans*, is a disease thought to be due to the presence of a peculiar parasite, the *microsporon Audouini*, in the epidermis and hair follicles. Its presence seems never to have been detected, though one does not well see why, on the naked skin; but it may affect "the scalp, eyebrows, beard, and genital organs," so as in fact to destroy all the long hairs of the body; and Dr. Anderson quotes an interesting but doubtful case from Bazin, in which it caused alteration of the nails. The disease still remains extremely obscure as to its nature; the parasite is seldom to be detected; though the reviewer must admit that he has been shown it, in hairs from a perfectly typical case, by no less sceptical an authority than Devergie of the St. Louis. The parasite may possibly be present only for a limited time; or perhaps the parasitic has yet to be distinguished from a non-parasitic form of the affection; in any case further researches are required to clear up the mystery of the question. The following is Dr. Anderson's description of the first symptoms of the parasite in the epidermis, and of its subsequent effects on the hairs:—

"On examining the hairs of the affected parts in the early stages, they are found to be dull and lustreless, and more easily extracted than healthy ones. The skin from which they proceed is found to be slightly reddened, and it is also the seat of a peculiar swelling, having the appearance as if the subcutaneous tissue was cedematous; but it does not pit on pressure as in true oedema. At this early stage there is a whitish matter on the diseased skin and hairs, which is nothing else than the parasite which occasions the disease. It is not, however, in nearly such abundance as in herpes tonsurans, a disease which it resembles in many respects. At the commencement itching is complained of, which is generally slight, so much so, indeed, as often to excite little attention, so that the disease is not observed, and continues its ravages till baldness has been produced. This first stage is of short duration, and is followed by the falling out of all the hairs of the affected parts, which takes place very rapidly, leaving a rounded bald patch. By this time the faint red tinge of the scalp has completely disappeared, being replaced, according to Bazin, by a condition exactly the opposite; that is to say, it has become perfectly white, having apparently lost every particle of its pigment. This white colour certainly contrasts strongly with that of the parts of the scalp provided with hairs; but it seems probable that this is not so much owing to any defect of pigmentary secretion as to the loss of the roots of the hairs. After the hairs have fallen out, they are replaced by a number of fine short downy ones, resembling the down on the cheeks of infants. By and by even this disappears, the slight swelling of the subcutaneous tissue subsides, and we now see merely the rounded patches of baldness, the skin of these parts being smooth and white. The disease is often very limited, there being only one small patch on the scalp; but more frequently there are several, and these, at first small, have a great tendency to increase and involve the neighbouring healthy parts by circumferential extension, so that at last a number coalesce, forming one large patch, having a serpentine form; hence the name ophiasis sometimes applied to this affection. In this way the whole of the scalp may be attacked, and permanent baldness produced."

With regard to the treatment of this disease by epilation, it

must be confessed that it is easy to say that it ought to be done, but very difficult to do it. To take out the hairs by the roots is a sheer impossibility; even Dr. Anderson prefers to blister, and leaves the dexterous French *épilleurs* to the difficult task imposed on them by theory. What am I to do, asks Devergie of Bazin in an imaginary conversation, "if there is not a hair where the malady is developed?" "Epilate," the latter is presumed to answer. "But the malady characterizes itself essentially by the falling out of the hair, so that the surfaces are left completely smooth, and there is nothing to epilate?" "Epilate." "But the hairs that remain are so mutilated, short, and brittle, that it is impossible to get them out with the forceps?" Still "epilate." In justice to Bazin, it must be admitted that his learned colleague makes him somewhat more unreasonable than he is in reality.

In the course of Dr. Anderson's remarks on *pityriasis versicolor*, he speaks of it as little liable to be confounded with *ephelis*, meaning by this term, not sun-spots only, but also *chloasma*; a pigmentation due to various derangements of various abdominal viscera. Now this last is unquestionably sometimes so like *pityriasis versicolor* as to render a certain diagnosis nearly impossible without having recourse to examining the invariably distinct and beautiful vegetation of the parasite with its tubes and clusters. Even Mr. Hunt has been struck with their appearances in *pityriasis lutea*, as he calls this disease. He first distinguishes two forms, the *contagious* and *non-contagious*, of which the former is the *least* common. Then he goes the length of admitting that the contagious variety "is probably communicated by the sporules of a microscopic plant." But he guards himself by recommending arsenic "to restore the tone of the cutaneous vessels;" and philosophically argues "that although this disease appears to be communicable by the transplantation of the fungus from one individual to another, it by no means follows that the presence of the parasite constitutes the disease." "I believe it only communicates it," he says. This is surely very like admitting, at any rate, that the parasite constitutes the cause of the disease; that it does not constitute any of its *symptomatic lesions* must be what Mr. Hunt means to say. Some of these it certainly does not constitute, for example *desquamation*, which is present in this disease; but others, such as the colour of the patches due to the visible presence of the parasite, are assuredly constituted by that presence just as much as they are caused.

Dr. Anderson has done little more than acknowledged the existence of a question as to the possible identity of the vegetable parasites belonging to different diseases, which seems to be

advocated to some extent by Professor Hebra, and one or two dermatologists of less note. A recent author in this country has even gone the length of stating that he has seen favus on one part of a human head, ringworm on a second, and alopecia areata on a third. Without absolutely declaring a statement of this kind to be false, it would surely be very rash in any one to believe it as a mere statement, even had it been put forth by a very eminent authority. Such a picture is certainly opposed to common experience; and the author may rest assured, that if he can prove the truth of it by proper description and accurate plates, and if he can show that there is no rare coincidence in the case, his name will be remembered for ever. But neither this author, nor any other practical men who have defended the doctrines of parasitic identity in this country, have given evidence from their writings, by which alone they are known, of being competent judges of facts.

Having already pointed out the practical tendencies of Dr. Anderson's book, it is matter of no wonder that the nomenclature adopted is, for the most part, one in general use; and it is quite possible that the author may object to it without feeling himself at liberty, in the mean time, boldly to overleap the obstacle. Yet it were to be wished that he had done so, the nomenclature being one that no reasonable person can defend. Three diseases at any rate, probably four, are shown on the most unexceptionable evidence to be manifestations due to the presence of vegetable parasites. These form together a group of diseases caused by *epiphyta*, as natural, and, with all due attention to Mr. Hunt and the sceptics, as perfectly demonstrated as dermatologists could desire. They have nothing to do with herpes, or pityriasis, or sycosis, as usually understood; from which diseases they were at once eliminated in reality, if not in name, by the very first observers who made the discovery of their true nature. The advance of science necessitates the recognition of new conditions, and new things must have appropriate names, to enable people to understand them. But the introduction of new names demands great discretion, an injudicious nomenclature being apt to bring any subject into temporary disrepute, especially among those only superficially acquainted with its details. Now, the following is almost a general principle. When an old group has to be split up, and a new one defined more accurately from a certain number of its members, it is rarely appropriate for the new group to be designated by the name of the old. It is naturally very rare for a subject imperfectly understood, to have received a name equally appropriate to the same subject when better understood. In the case of skin diseases generally, this rule has long been recklessly

disregarded, to the obvious confusion, not only of students and practitioners, but even of accomplished dermatologists, who are often so predisposed in favour of names, as to disregard the essential phenomena, which names ought merely to express. The time cannot be far off when such considerations must make their impression, and when such words as *tinea*, *herpes*, *sycosis*, and *pityriasis*, must cease to be employed as descriptive of parasitic affections. It would be very hard for any one to tell what *tinea* means at the present day, or even what it ought to mean. Alibert and his predecessors used it pretty generally for the different varieties of scalp-eruptions; as expressive, like the *porrigo* of Willan and Bateman, of a multitude of diseases. But if one affection more than any other has always been ranked as a *tinea*; if there be, as Bazin calls it, a "teigne par excellence," it is surely favus; a disease which Dr. Anderson's nomenclature, however, carefully distinguishes from *tinea*. Dr. Anderson's own *tinea tonsurans* is subdivided into two varieties of *herpes*, and a *sycosis*. *Herpes tonsurans* is easily understood as a variety of *tinea tonsurans*, since by most people the two would be looked on as synonymous expressions; but to enumerate a *herpes circinatus*, an affection of the body, as a *tinea*, which is an affection of the head, is surely a liberty to be avoided on the author's part, even though it may be sanctioned by the precedent of Bazin. The whole history of the word *herpes* is worth a moment's attention. It was used by the older dermatologists to express any chronic cutaneous malady; the *herpetes* of the ancients being nearly equivalent to the *dartres* of the French. The school of Willan, with a view to making the term more precise, characterizes as a *herpes* an eruption of vesicles in clusters. Another school, with M. Devergie of Paris at its head, means by *herpes* an eruption in a circular form. Then it was discovered that a number of these circular eruptions were parasitic; and some authors, Dr. Anderson among them, seem to wish to restrict *herpes* to parasitic forms. Thus, altogether, between clusters of vesicles, and circles, and parasites, an extraordinary confusion has arisen, which has destroyed to a great extent the usefulness of the word. Perhaps Hardy is right in the circumstances in wishing to discard it altogether; only he has not himself had the courage to do this; giving in his work the nomenclature which Dr. Anderson has adopted, with the single exception of his employing, in place of the questionable general expression *tinea tonsurans*, the French term *Trichophytic*, for the affection caused by the *trichophyton*. As for *sycosis*, the term has long been in use for a tubercular eruption, analogous to acne, occurring on hairy parts. That the now well-established parasitic form ought to be separated from the

other, few will question; but it is quite otherwise with regard to the propriety of the course which Hardy and Dr. Anderson have followed in retaining the expression *sycosis* for the parasitic form, of which it is in no degree suggestive, and which necessitates such an innovation on the common use of words as the doctrine, that a tubercular affection of the skin, if not parasitic, is no longer to be called a *sycosis*. Much more rational, on principles already suggested, is Herr Köbner's proposition to give a new name to the parasitic form, retaining *sycosis* to express the disease to which the term was in great measure originally applied, and with regard to the nature of which the ideas of dermatologists are little modified. Still more rational would it be, on account of the greater precision arrived at, to dismiss *sycosis* altogether from strictly scientific language, and to distinguish the parasitic affection from an *acne sycosiformis*.

There is another parasitic disease which almost every clinical physician has daily opportunities of accidentally observing; the parasite of which is lodged in the epidermis, and of remarkably easy demonstration. This, while an obvious member of the same natural family as favus and ringworm, is neither called *herpes*, nor *tinea*, nor *porrigo*, but, with an incongruity calling loudly for reform, *pityriasis versicolor*.

The strictures of the preceding paragraphs would lose much of their force if there were no new nomenclature to suggest better than that which, in Dr. Anderson's work, is now claiming a recognition in England, which, it is sincerely to be hoped, will not be too hastily accorded. That adopted by Köbner in his brilliant and almost exhaustive article is due to Virchow, and possesses every advantage as simple, scientific, and suggestive. Parasitic vegetations on the skin, hairs, or nails, he proposes generally to call *mycoses* (μύκη, a fungus). Thus, leaving out the doubtful alopecia areata, the botanical position of the parasite in which is by no means beyond question, *favus*, due to the *achorion*, would be "mycosis achorina;" *ringworm*, due to *trichophyton tonsurans*, "mycosis tonsurans;" *pityriasis versicolor*, due to the *microsporon*, "mycosis microsporina." Favus further affects the epidermis, the hairs, or the nails; it is, therefore, either a *dermatomycosis*, *trichomycosis*, or *onychomycosis*. The same remark applies to ringworm; even an *onychomycosis tonsurans*, which seems to have escaped Dr. Anderson's attention, being recorded by Köbner. Pityriasis versicolor remains of course a *dermatomycosis*. This simple and scientific nomenclature seems to answer every purpose; the reviewer trusts it will not be long before it is everywhere adopted.

Dr. Anderson's book is illustrated with beautiful woodcuts, which do great credit to the artists of the city in which they

were executed. Matter from a favus crust (p. 20), a hair from herpes tonsurans (p. 67), and above all, the representation of a crust from a case of scabies norvegica (p. 135), are all that could be desired. The large clear type and praiseworthy taste evinced in its getting up, are in good keeping with the sterling value of its contents.

That Mr. Hunt's book has reached a fifth edition, is little to the credit of dermatology in this country. Finally, on the merits of the article by Heinrich Köbner, much, but not more than enough, has been said in the course of the review.

II.—*Another Letter to a Young Physician.* By JAMES JACKSON, M.D., &c., 16mo, pp. 179. Boston: Ticknor & Fields, 1861.

THERE seems to be a tide in the opinions and thoughts of men, as well as in their affairs; a periodical flux and reflux in the intellectual as well as in the physical world. Slower they are, no doubt, and a term of centuries may be embraced between the extreme points, but they are none the less real and certain. It would seem as if an irresistible law of compensation prevailed, carrying away the reason of the age or party, and tossing and tormenting minds with doubts and fears, as if they were novel, and had not tossed and tormented other minds hundreds of times before. Great movements, or doctrinal discussions in the church; prophecies of national decadence, or trumpeting of progress; party opinions in politics; schools in logic and metaphysics; theories in the principles of medicine; drugs and courses of treatment in its practice, are all instances in point. It is undeniable that this pendulum-motion of intellect is injurious by preventing progress. Force, which should carry right onward, merely repeats itself and does no good. How then does it arise? In matters of sentiment one extreme naturally tends to its opposite, as a welcome relief, but in matters of intellect, how comes it that there is sometimes a fashion, a cut of opinions, so to speak, varying like the cut of our clothes, though at wider cycles? Here we are now, after an experience coeval almost with the existence of man, discussing the question for the hundredth time, is there any virtue in the art of medicine? can good be done by the administration of drugs? The question is not put by the public. They have implicit faith in our nostrums. It is put by members of the profession. The fault must therefore lie with the profession, and cannot be explained by popular ignorance or presumption.

It would be interesting to discuss the question, Does the administration of drugs do good, in the *à priori* method? No one denies that they may do harm, and we believe that it follows as a corollary, that they may do good, that they do good, provided only we know when and how to administer them. But, since our object is to discourage *à priori* reasoning in any branch of medicine, we proceed at once to state our belief that all these revulsions of opinion as to matters of fact are due to want of attention to the rules of right reasoning. This is the ultimate source of those doubts as to the efficacy of therapeutics, and those vibrations from theory to theory, and specific to specific in the treatment of disease. The chain of sequence is this:—theories, physiological or pathological, formed from insufficient induction, or none at all; remedies founded on these theories or on narrow experience; these remedies tried by others and found wanting; men so disappointed concluding, naturally perhaps, but illogically, there are no such things as remedies, no such things as even palliatives; the doctor is a mere spectator of the practice of the great medicatrix, Nature. Having this belief, we may be pardoned if we devote a few lines to the consideration of the logical conduct of medicine and especially of therapeutics. To many, what we have to say will be the utterance of a series of truisms; but this they will excuse for the sake of others, more especially our younger readers.

The distinction between science and art, or between an art and its science, is a fine one, but one which may be made pretty evident. "Art and science differ"—says Whewell.* "The object of science is knowledge; the objects of art are works. In art truth is a means to an end; in science it is the only end."† So much for their individual objects. What are their mutual relation? "Inductive truths are of two kinds, *Laws of Phenomena*, and *Theories of Causes*. It is necessary to begin in every science with the laws of phenomena; but it is impossible that we should be satisfied to stop short of a theory of causes."‡ Science, any science has two stages, standing in a necessary

* "Philosophy of the Inductive Sciences," vol. i. p. xli.

† We may accordingly give these definitions:—

The *Science of Medicine* is the knowledge of the causes, nature, and tendencies of disease, and the rationale of its treatment.

The *Art of Medicine* is the practical discrimination and treatment, prophylactic or remedial, of disease.

The definitions given by Dr. Aitken in his Handbook are very confused and interwarped. It is no objection to the principle of our division, to say that the objects of medical science are works. If we analyze our conception of a science, we find that the idea of works is not essential to it.

‡ Ibid.

relation in time; the finding of the laws of phenomena, and constructing the theories of causes. The common ground of an art and its science is the laws of phenomena, where in the beginnings of science, they meet, but need not coexist; for, even here, they soon part company, science advancing to generalizations. In short, it is from the experience of the art that its science draws its first facts. Perfect science teaches both *what* takes place in any phenomenon, and *why* or *how* it takes place. To know the *why* strengthens our grasp of the *what*, and gives confidence power and wider range in its application, but is not necessary to it. Perfect science, therefore, is necessary to perfect art; but "art, in its earlier stages at least, is widely different from science, independent of it, and anterior to it."* Illustrations lie on every hand. The art of fermentation is perhaps the oldest on record; yet the science of fermentation is of most recent date, and is even now disputed. The facts of the science must, to a great extent, have been known before the proper practice of the art, *i. e.* that at a certain temperature, in a certain time, both in the first place, only guessed, the juice of the grape underwent peculiar changes; yeast produced peculiar effects upon dough, &c. But as to its laws and the theory of its cause, there was no science until Liebig's beautiful investigations; and now doubts are being thrown in some quarters on his results. Yet brewers brew, and bakers bake with as much confidence and success as ever. In medicine, illustrations are strewn "thick as leaves in Vallombrosa." Take vaccination as the simplest. We know little of the laws of its phenomena, and nothing of their cause. If we did, the knowledge would extend, and make more certain the art, but we do not the less practice the art because of the imperfection of the science. We say the same of every disease and every agent in *materia medica*. We must know something of its phenomena; but we need know nothing of their theory, in order to rational treatment. Facts, therefore, are at the base both of art and science. It is an old and commonplace saying, but one which, to the shaking of our own belief in medicine and to the injury of our patients, we constantly forget. Facts are necessary, but facts not promiscuously noted without regard to their relations, but with time, place, constitution of patient, stage of disease, and all the *et ceteras*, insignificant in seeming, but which will at once suggest themselves to an accurate observer and close reasoner, as being essential to the progress of the art, and *a fortiori* to the discovery of the science. This is what Whewell means by his fourth "aphorism concerning science." "Facts are the materials of science, but all facts involve ideas. Since in

* Whewell, vol. ii. p. 276.

observing facts we cannot exclude ideas, we must, for the purposes of science, take care that the ideas are clear and rigorously applied." But here lies the difficulty which belongs pre-eminently to the collection of facts in medicine; the multiplicity of the concomitant ideas or relations, their uncertainty, delicacy and variability. We study a living organism, having a compound nature, whose internal economy is hidden from us; varying from hour to hour in tendency and susceptibility even in the same individual. The type of the same disease varies not only periodically in the mass, but in different individuals in the same period, and in the same individual at different periods. "We doctors," says Dr. Jackson in a previous work, "like the tailors, find that coats of the same size, and proportions, and fashion, will not suit every individual." Hence, we cannot treat diseases but symptoms, and are even here many a time disappointed and baffled by seemingly opposed results.

Having got our facts thus sifted and tested, we make the first step in science, we gain a knowledge of the laws of these facts, and therewith a previously unattained measure of assurance, remembering that absolute certainty we never can have in the diagnosis and treatment of disease. And now we may grope after science properly so called. We may experiment and reason as to the why of these laws; the various steps of the process between the agent and the effect, pathological or therapeutic. This must be done, not rashly, or wildly, coupling together phenomena by any figment of our imagination, or any chemical or mechanical relation, forgetting that the sphere of action is vital, like the "*iatro-mecaniciens*" or "*medecins algebristes*" of the seventeenth century. Nor must our art be regulated by these changing efforts at a science, until they have passed all the ordeals of right reasoning. If it be, our art will change with the changing theory; we shall be led from the safe path of observation and experiment into one which has no surer base than air, and no character but that which the tendency of the age impresses upon it.

In what respect do we sin against these rules, and how is our sinning connected with our failures, and that fickleness and doubt from the consideration of which we started? We sin in two ways, by studying medicine either wholly as an art, or chiefly as a science; by being either *artisans* or *savans*. There are, of course, all gradations between these extremes, and the proper combination of both, the genuine scientific artist, the ornament and stay of our profession. The perfect medical artisan begins of course as an apprentice—"the doctor's boy." From the lighter jobs of the art he gradually rises to more intricate undertakings. He sees his master give such a drug in such a case, and he imi-

tates him in the next. It cannot be wondered at that he should make mistakes. We know how dangerous it is to have a fact in the head without an idea attached to it. Mr. Newcome hears the answer, "Scrape the ceiling with the fire-shovel" praised, as showing ready invention in a case of poisoning by oxalic acid. He takes note of it, and when asked—"You are sent for to a man who has hung himself. What would be your first endeavour?" Answers—"To scrape the ceiling with the fire-shovel!" This is the lowest grade of artisan; but there are others who study the laws of phenomena, but affect to despise all theorizing or attempts to discover their science. These are the "practical men." Many more in these modern times err on the side of science, and their number will vastly increase under the new regime of medical education, unless the danger is held out prominently. The professional knowledge of the medical savan is based upon an excellent general education. Man is an all-related being; the relations of medicine must be as universal, and he can bring all the sciences to bear upon it. In practice their diagnosis is finical; and a patient is rather a field of discovery, and for the exhibition of chemico-physiological phenomena, than a fellow-being enduring suffering which it is his business to remove, or at least relieve. When he treats he can render a reason—grand scientific principles; improbable physiological theories; romantic chemical decompositions. He trims facts to suit a theory already formed, and then from this theory deduces plans of procedure, which having a basis so logical cannot fail to be successful. We were told the other day a good story about a distinguished Edinburgh professor, who has arrived at the treatment of inflammation after this fashion. He himself became the subject of treatment, for some inflammatory affection, and, horror of horrors, his medical brethren determined that bleeding was necessary. Theoretically, he ought to have died, but practically he rapidly improved from the practice, and flourishes in spite of himself and his theory. Dr. Jackson furnishes us with an excellent illustration of practice founded upon a theory formed without facts, and its haphazard results.

"In this city, during the life of Dr. Rush, there was one practitioner, regarded by many as the most scientific man among us, who abstained from blood-letting entirely, or nearly so. This was Dr. Samuel Danforth, a man with a strong mind and a strong will, but wedded to a theory. It is singular that, while there was a great similarity in the doctrine of these two gentlemen, there was none in their practice. Like the old *methodics*, and the late John Brown of Edinburgh, both of them regarded disease as differing from health in more or less of something. If health was thought to be maintained by a certain amount of tension in the living fibres of the body, then it was thought that disease consisted in more or less tension, more or less excitement, than appertained to health. This description is sufficient in a general view of the systems of these various medical philosophers. Some of them, Rush especially,

regarded disease as a unit. But between Rush and Danforth there was this difference. Rush believed, that, in almost all instances, disease consisted in too much excitement. Danforth, on the contrary, maintained that it consisted almost always in too little. Hence, Rush almost constantly prescribed blood-letting, that he might diminish the amount of the most general stimulant, one which pervades, or at least has an influence on, every fibre of the body. In a very large proportion of febrile diseases there is experienced after bleeding, some relief, for the moment at least, as to pain and distress. There may be, at the same time, a feeling of exhaustion, and not uncommonly syncope ensues. But then this is treated as a temporary inconvenience, making ultimate relief more certain. Hence the sick were led to submit to this treatment, and to regard the evacuation as useful. The other gentleman, Dr. Danforth, looking at debility as the great source of disease, administered diffusible stimulants, with an idea that, if they do not give permanent strength, they bring on the normal actions, by which the strength may be renovated. He also looked ultimately to nutrition as furnishing the substantial means of restoration. With this view he took care to relieve the alimentary canal from any undue burden, and from any offensive materials, which he supposed it might contain. When thus prepared, he believed that the patient might be brought back to a normal condition, sooner or later, by furnishing him with proper nutriment, and adding to it direct stimulants, by which all the organs, but the digestive organs especially, might be driven to the performance of their proper functions. At the same time he paid great attention to some other hygienic laws, thereby favouring the influence of good diet. In a large proportion of chronic diseases, the patient derived, at least, temporary comfort from this treatment; and when pursued with judgment, and where no organic disease embarrassed the case, he often obtained from it permanent relief. In acute diseases relief followed in a greater or less degree, from the early evacuations of the alimentary canal, which he thought a necessary preparation for his other treatment. But the plan of supplying nutriment, and of using diffusible stimulants in acute diseases, sometimes occasioned much trouble and distress. However, Dr. Danforth was ordinarily too sagacious to press this treatment very strongly, when the patient's feelings forbade; then he waited and watched his chance. In this way he often succeeded."

Were it necessary to choose between an artistic and a purely scientific medicine, we should prefer the former. It would do less harm to the public, and hold out brighter hopes for the future of medicine. When we speak of an artistic medicine, we do not mean that unthinking form which we described as an ultra-illustration; but that which is wholly devoted to the what and despises the scientific why of practice. Indeed there are few men who begin as mere medical tradesmen,* who do not naturally, as their life advances, grow into the higher stage, and acquire by gradual aggregation and digestion of facts, some accurate knowledge of the laws of phenomena, and some scraps of real scientific theory. They at any rate lay a foundation. But the man who thinks out in his study, or works out in his laboratory, or even in the inspection-room, insufficient theories, upon

* We had almost said "medical men." When will this inaccurate and stupid phrase be abandoned? We ought to speak of "legal men," and "theological men." We once heard the devil defined as a "theological animal."

which he or other men found therapeutics, does no good to any one, whatever harm he may do. Certainly he does harm, both by causing false treatment, and satisfying for a time the minds of observers who would otherwise prosecute inquiry. Rather let us make our daily "tale of bricks" for the use of the master workmen of the after ages. The office is humble but honourable.

We have been led to make these remarks by this "Letter to a Young Physician," additional to a series published by Dr. Jackson, some years back. The form of letters is rather unfortunate, as being perhaps the most unnatural under which a writer could bring his ideas before the public. We have a wholesome horror of "Letters" to anybody, just as we should have were they actually addressed to us. Such a title merely gives one a prepossession against the book, and leads the author into a loose mode of expression, the only quality of ordinary letters they can be said to possess, excepting an occasional directness of address to an imaginary individual. All this, however, is of minor importance; the entire series possess intrinsic worth. Dr. Jackson is a model of a physician. His tendency is rather too much to the artistic side, but on the whole he is a fair example of what the patient experimentalist and accurate observer can do with his art. In the previous series he jots down a number of valuable facts in treatment, which his experience has certified. Now this additional letter has been elicited by one of those periodic outcries against the utility of medicine to which we at first alluded. It is comforting to the young practitioner to hear one of the Nestors of medicine declare, after about sixty years' experience, his belief in the efficacy of his craft unshaken. It is anything but agreeable or heartening to one just entering the fight, to have his weapons declared useless. But Dr. Jackson believes in the efficacy of the art only when practised, subject to those rules on which we have already dwelt at such length. Indeed, the whole book is a reiteration of them, only not in technical language. The general tenor of the book, the spirit of Dr. Jackson's philosophy, and its agreement with our own, may be gathered from a few side-speeches, as it were, thrown in here and there.

"I mean only to add my testimony to that of others. I will not waste time in discussing the *modus operandi* of these medicines. On all practical questions, if others will agree with me as to the results produced by any given mode of treatment, I am satisfied without any such discussion.

"Those, who hold the favourite opinion of the present day, that acute rheumatism has its origin in a morbid state of the blood, will readily explain the failure of relief from bleeding. But questions regarding the proximate cause of disease I am not fond of discussing. For me, the experience gained from care-

ful and honest observation, is more satisfactory than any the most plausible and most ingenious hypothesis.

"By leaving a case to nature, it is not meant that we must leave it to chance.

"To show the claims of medicine to the character of a useful art, I have given an account of the uses which may be made of some of the most powerful drugs employed among us at the present day. Subsequently, I pointed out some of the most common diseases, in which I have thought medicinal drugs, as well as other treatment, may be used with benefit. I have given the results at which my own mind has arrived. I have not pretended to demonstrate the justness of my conclusions. Nor have I brought forward any system of medical science. I am not capable of so doing. I think I may say more:—viz., that our knowledge at the present day does not justify the attempt to bring forward any such system. I do not deny that we have much valuable knowledge in the various branches of medicine. I maintain the contrary opinion. But, as I stated in the first part of this letter, our acquaintance with physiology and pathology is limited and imperfect; and these are the fundamental branches. Of course the same must be true in regard to therapeutics. We do not know enough, as to either of these branches, to enable any one to offer a satisfactory *system of rational medicine*. In many instances, certainly, our practice is founded on experience alone. In the good sense of the word, our practice is *empirical*. Happily this is often sufficient. If we know that cinchona will arrest an intermittent fever, have we not good cause to be satisfied, although we cannot answer all the questions, which may be put, in regard to the etiology and the pathology of that disease, or in regard to the *modus operandi* of the cinchona."

We had marked many passages of a more practical nature for quotation; but we have already so far over-stepped our space, that we must be satisfied with merely assuring our readers, our younger ones especially, that the book and its predecessor are worth their attention. The statement of the evils and advantages of blood-letting, is perhaps the best specimen of Dr. Jackson's calm and discriminating judgment.

III.—*A Practical Treatise on the use of the Ophthalmoscope*. By J. W. HULKE, F.R.C.S., &c. London: Churchill, 1861. 8vo, pp. 70.

THIS short treatise will go far to supply a want which has been much felt in this country by those engaged in the study and practice of ophthalmic medicine. Although numerous articles on matters connected with ophthalmoscopy have appeared in our medical journals, and though we have had the little practical work of Mr. Hogg for several years, this is the first English book on the subject, so far as we are aware, in which a regular systematic treatment both of its optical and pathological details has been attempted.

The British student has now the requisite elementary knowledge laid before him in a convenient form and in his own language, instead of being obliged to pick it up, as heretofore, by a troublesome and uncertain process of inference from remarks in which it is taken for granted, or by the perusal of foreign works.

Mr. Hulke's work comprises his Jacksonian Prize Essay for 1859; but he has both extended the range of subjects treated, and condensed his matter, in order to adapt it to the purpose required—that of serving as a guide to the ophthalmoscopic examination of the eye. The author justly remarks in his preface—

“Notwithstanding the progress which has been made in ophthalmoscopic diagnosis, very much remains to be done before anything like a perfect work on the subject can be presented; and in this which I offer merely as an elementary treatise some points are necessarily only just touched.”

Taking this into account, it would be unreasonable to expect anything very new or original from the book; and although persons familiar with the ophthalmoscope might desire to have a fuller exposition of various points from the pen of one so favourably known as a careful and experienced observer, we must recollect that it is most suitable for the author of an elementary treatise to confine himself to the consideration of such pathological facts and phenomena as are generally admitted and recognized.

At the same time, the book is something very different from a mere compilation, as the well-arranged and carefully-described original cases contained in it testify.

The first part, illustrated by twelve diagrams, treats of the optical part of the subject; and we must congratulate the author on the success with which he has condensed his account of it within the space of twenty pages, without either rendering it irksome to the reader from its very succinctness, or sacrificing general accuracy to brevity; a task the difficulty of which can only be appreciated by those who have tried it.

The diagrams give a very good idea of the action of the various instruments represented in them; and, although the hypercritical might object to certain trifling defects—such, for example, as the position of the optical centre of the eye, which is placed in the neighbourhood of what the Germans term the *Hauptpunkte* rather than in that of the *Knotenpunkte* where it ought to be—they are of no sort of consequence with regard to the principles which Mr. Hulke finds occasion to illustrate; his object being rather to give a general view of the rationale of different instruments, than to furnish a minute or comparative analysis of their various properties.

The following are the only critical remarks which we have to offer on this part of the work ; and it will be seen that they do not refer to matters of much moment :—

At page 7 Mr. Hulke speaks of an “ erect, virtual, geometrical image which *may lie in front of the eye examined*” as being seen by the observer employing the direct mode of examination, and using a concave lens as eye-piece. The case is illustrated in fig. 12. This is quite a conceivable state of matters ; but it need hardly occur except in the case of a very myopic observer, A, who has strong objections to being in close proximity with his patient, B, and “ whose field of vision,” to use our author’s own words in page 6, “ would be so much limited by the (apparent) smallness of B’s pupil,” consequent on his distance from it, that we should advise him to prefer the indirect method of examination.

In fig. 8, the letters *a'* and *b'* ought to be transposed.

We may remark, in regard to some statements in page 20, that the main advantage of such instruments as those of Zehender or Coccius is, that a larger portion of the patient’s retina can be illuminated by means of them at short distances than by any ordinary concave speculum. This is the case whether the outer margin of the patient’s orbit comes in the way or not—and really it need never be in the way if the lamp is properly placed.

The second part contains an account of the appearances of the healthy structures of the eye when viewed ophthalmoscopically.

At page 22 the optic nerve-entrance is described as encircled by a double ring ; and it is added that—

“ The two rings are the apertures in the choroid and sclerotic, the former of which is the smaller, and constricts the nerve more closely. In consequence of this disposition of the parts the extremity of the nerve is first encircled by a belt of choroid, and then beyond this by a narrower white ring, the edge of the sclerotic foramen. The presence of an excess of pigment in the choroid ring, produces the common appearance of a black, or sepia-coloured, incomplete circle, which is no sign of disease, but merely a peculiarity of development.”

So far as our own observation goes, we are inclined to give a somewhat different account of the appearances presented by the optic disc. Beginning at the centre, we have a white spot referred to by Mr. Hulke in a subsequent paragraph. It is of somewhat variable size and form, and corresponds to the place at which the retinal vessels pass through the lamina cribrosa ; the tissue composing the latter having a great reflecting power, and being apparently of a closer texture around the vessels than in the neighbouring parts.

The greater part of the disc outside this has a greyish colour, which, on closer examination, is seen, under favourable circumstances, to be due to the admixture of the dark neutral tint of the

interstices in the lamina, which are filled with comparatively transparent nerve-substance, with the white reflection from the tissue of the lamina itself. This grey tint often appears darkest near the margin of the disc; but, in most cases, an increased brilliancy of reflection is observed at the very edge of it. *Outside* this again, and very strongly marked, is the choroidal limit, frequently charged with an excess of pigment.

These appearances, as is well known, are more or less easily recognized according to the degree of transparency of the parts anterior to the lamina, which differs even in normal cases.

Liebreich* gives a similar account of the disc, and states that the white portion *internal* to the choroidal limit, and termed by him "limite seléroticale," is to be ascribed to light reflected from the tunic of the optic nerve at the place where it is continued into the sclerotic.

At first sight this seems to contradict Mr. Hulke's statement that the choroid aperture is smaller than that in the sclerotic, supported as he is by the formidable testimony of H. Müller; † and it must be recollected that there are sources of fallacy in the microscopic examination of hardened eyes, as well as in the ophthalmoscopic examination of living ones. Nevertheless it may be quite possible to see the sclerotic margin, or the wall of the canal in the sclerotic about the place where the lamina cribrosa and the tunic of the optic nerve join it, on one side of the disc at a time, by looking obliquely past the edge of the narrower choroidal aperture.

It may be remarked, too, that the white reflection is generally observed, at any one time, as a narrow crescentic line at one side or other, according to the direction from which it is viewed, and seldom, if ever, as a complete ring in the normal state of the parts; while, in cases in which the neighbouring parts of the choroid have become atrophied and translucent from disease, this crescentic line or ring is seen to be continuous with, and to form the margin of the wider crescent, or ring, formed by the portion of the sclerotic now visible.

But, on any assumption, we do not understand how the edge of the sclerotic foramen can be seen, as one would imagine from Mr. Hulke's description, through the choroid; and this at a part where, in normal eyes, the latter is at least as well supplied with pigment as in others where it completely masks the colour of the sclerotic.

No doubt there is a comparatively dark zone *within* the bril-

* Del'Examen de l'oeil au moyen de l'Ophthalmoscope. French edition of Mackenzie on the Eye. Vol. II., vide pp. xxvii. and lv.

† Archiv für Ophthalmologie. Bd. iv., Abt. ii., S. 3.

liant white ring ; but we see no reason to take this for the choroidal margin, or to doubt the explanation of Liebreich, who describes it as the limit of the proper nerve-substance. The true limit of the vascular and pigmented structures of the choroid we conceive to be always the extreme boundary of the contour of the disc as it appears in the normal state ; and, although we may admit that parts which are really exterior to it and in a plane posterior to it may be seen in perspective *apparently within* it, we do not think that such parts, if seen at all, could *appear* to the *outside* of it.

The third part of Mr. Hulke's book is occupied by descriptions of the appearances observed in diseased and congenitally imperfect eyes, and it forms an admirable compendium of ophthalmoscopic pathology.

The second and third parts are illustrated by four plates containing nine coloured figures—some of them reduced from Jaeger, and others original. Fig. 2, Plate II. is perhaps rather defective in the colouring ; but they are very creditable productions on the whole, when we take into account the difficulty of procuring such figures at all, and of publishing them at a reasonable price.

We have little occasion to enter into a further criticism of details, because there is no particular of much practical importance with regard to which we differ from our author ; and we will venture to say that he has omitted few, if any, established facts which it would be at all suitable to introduce to the notice of beginners, for whom the book is specially intended.

The use of the ophthalmoscope cannot be learned from books alone ; and no author can anticipate all the possible sources of error and perplexity which may beset an observer ; but, in so far as a book can contribute to the acquisition of such elementary knowledge as will enable the reader, in the exercise of his own intelligence, to detect them for himself, we think that Mr. Hulke's treatise is calculated to be eminently useful.

IV.—*Observations on the Result of Treatment of nearly One Hundred Cases of Asthma.* By T. L. PRIDHAM, M.R.C.S., M.S.A., &c. Pp. 62. London: J. Churchill. 1861.

MR. PRIDHAM has an extensive practice in Devonshire, and has, we believe, gained himself a reputation for his success in the treatment of asthma. This brochure, which is a reprint of papers lately published in the *British Medical Journal*, contains his views on the nature of that disease, and an account of the treatment he

has so successfully adopted. He has had nearly one hundred cases of asthma under his care, and twenty-one of these are recorded here with very favourable results. We confess to a feeling of disappointment at not learning the issue of the other seventy-nine. Probably he was not so successful with these. Yet a treatment which is found useful in twenty-one per cent. of cases of this intractable and distressing disease deserves attention, more especially as it is both simple and safe.

Out of the number of cases he attended, he found hereditary disposition in nine cases out of ten, and the exciting causes of the attacks were as follows:—Imprudent diet; an attack of bronchitis, or of influenza; derangement of the liver, or any other functional derangement; atmospheric influences; peculiar odours; and influences of the mind either for joy or sorrow. The most frequent of these exciting causes in his experience, is imprudent diet; and when the disease owes its origin to this source, it receives the name of dyspeptic asthma. All the cases recorded here, with the exception of two, belong to the variety of dyspeptic asthma, and the treatment he recommends is specially adapted to improve the state of the stomach and soothe the nerves. We shall quote the treatment of the first case, which is a good example of the treatment followed in all:—

“After giving the patient an alterative pill and a saline aperient, I ordered him the following diet, which was to be regularly weighed out to him, and the hours of meals most strictly attended to:—Breakfast at eight o'clock—half a pint of green tea or coffee, with a little cream, two ounces of stale bread; dinner at one o'clock—two ounces of fresh beef or mutton, without fat or skin, two ounces of stale dry bread, or well-boiled rice; three hours after dinner, half a pint of weak brandy and water, or toast water *ad libitum*; supper at seven o'clock—two ounces of meat, with two ounces of dry bread. He was not allowed to drink within one hour of his dinner or supper, or till three hours after; at other times he was not limited. Open-air exercise was ordered to be taken as soon as the office of digestion had been performed, but short of fatigue. In addition to this, I ordered him three grains of the extract of conium four times a day, at the hours of seven, twelve, five, and ten; the dose to be gradually increased to five grains four times a day.”

It is easy to understand how the foregoing treatment should be beneficial in those cases where the attacks have been brought on by overeating or imprudent diet. Removal of the cause is recommended as the first step in treatment of all diseases, and Mr. Pridham applies that principle very effectually in the cases before us. We have an impression that the starvation is perhaps carried too far in those cases. The treatment would certainly be more willingly submitted to by the patient, and probably would be equally successful, were the allowance of bread and meat increased a little.

The sedative extracts used by our author in addition to that of conium, are those of henbane, belladonna, and stramonium, and he insists on attention being paid to the purity of the drugs, as he has been frequently disappointed in the result of treatment by the inferiority of the articles supplied by the druggists.

The other forms of asthma are briefly alluded to, and are treated by Mr. Pridham in the ordinary way, by removing the exciting cause of the attacks whenever that is possible, and palliating the symptoms by antispasmodics and sedatives.

We recommend the perusal of this pamphlet to all who feel interested in the subject of asthma.

V.—*Lectures on the Diagnosis and Treatment of the Principal Forms of Paralysis of the Lower Extremities.* By C. E. BROWN-SÉQUARD, M.D., F.R.S., Laureate of the Institute of France, &c. 8vo. Pp. 118. London: Williams & Norgate, 1861.

WE have already on more than one occasion directed the attention of our readers to the physiological labours of Dr. Brown-Séquard, so perseveringly engaged in, and attended with such brilliant results. The last time we did so, we concluded a review of such of the many essays and fragmentary contributions from the pen of this able physiologist as had come under our notice, with the remark "that it would be of great service to the science in which he is so able and laborious a pioneer, if he collected his numerous experiments and observations, and gave to the world a more systematic work on the physiology and pathology of the nervous system." Scattered as they were, and that to a great extent throughout the scientific periodicals of England, France, and America, it was then no easy matter to realize the avowed opinions even of so eminent a physiologist; and it was therefore with much gratification that we hailed the appearance of two works by Dr. Brown-Séquard, in which the wish formerly expressed by us was realized, and realized in a manner which places the already high reputation of their author almost beyond the reach of hostile criticism, while the works themselves will prove an abiding monument to his talents and industry. Had he trusted to the scattered fragments to which we have already alluded, as a foundation for future fame, we feel assured that his reputation would never have been fixed on that enduring basis which he has now established in works which will rank, along with those of Magendie and Bell, among the classics of physiology.

The more important of the two works, being the substance of a course of lectures delivered at the Royal College of Surgeons in May, 1858, was published last year in America, but, owing to a mistake, did not reach us until quite lately. This we mention in order to explain how it is that we are now about to notice the later work before its predecessor, although the former may in some measure be regarded as a pendant to the latter. The subject, however, of the work which we now have more immediately under our consideration, is one which may with perfect propriety be examined, without reference to the other works of its author; and which, while it is of vast importance, especially in a therapeutic point of view, can be treated *per se*, assuming only that the reader has a certain knowledge of the laws which regulate nervous action. Indeed, we are by no means sure but that a careful study of the opinions and mode of reasoning of the author, as applied to a limited section of the subject, will enable the reader more thoroughly to appreciate and more readily to understand when he comes to analyze the more elaborate and systematic work.

We use here the word "analyze," advisedly. Any one who has taken up a book in which an abstruse scientific subject, such as the physiology of the nervous system, is treated of in a manner new to the reader, knows that no mere *reading*, in the ordinary acceptation of the term, can suffice to place clearly before his mind the reasoning and conclusions of the author, in such a form as to deduce accurate notions respecting the validity of the one and the other. Such a book requires study, if you would do justice to the author, or even to your own powers of observation; and we can assure our readers at the outset that the works of Dr. Brown-Séquard call for constant attention, and a perusal of them will only repay him who takes the trouble to think, as well as read. It is far from our intention, however, to insinuate that the reader will here meet with greater difficulty than is inseparable from the subject. On the contrary, he will find that the author possesses the rare faculty of combining propriety and even elegance in his style with a terse and vigorous clearness; and that his explanations are for the most part far from being ambiguous or indistinct, but are, on the contrary, so expressed as to guard against any possible chance of misapprehension.

We had the privilege in 1859 of attending a short course of lectures by Dr. Brown-Séquard, in which the subject of paralysis of the lower extremities was, among other things, briefly treated of. In extension of this part of his subject, he has now given us a treatise upon paraplegia, in which no question seems unanswered, no noteworthy opinion unnoticed. The main object

of the book appears to have been to establish beyond a doubt that such an affection as "reflex paraplegia" exists. To this subject two of the four chapters of which the book consists are entirely devoted; and we shall here briefly consider with our author the subject to which he has devoted so much time and attention.

Dr. Brown-Séquard lays no claim to being the first to recognize and describe cases of reflex paraplegia; but, on the contrary, he gives all honour to Graves, Romberg, and others, who certainly detailed such cases, although they were far from appreciating what our author now describes as their true pathological characteristics. The existence of a reflex paralysis is, in so far as the upper part of the body is concerned, a fact generally, if not universally, admitted; but, as regards the inferior extremities, the case is different. While many men of eminence—Graves, Davaine, Romberg, Stanley, and the like—have adopted opinions which coincide more or less closely with those of Dr. Brown-Séquard, it must be conceded that no inconsiderable number of men, whose opinions are entitled to the greatest respect, have ranked themselves on the other side. Of late years, indeed, these latter have shown a formidable front, and, among others, Dr. W. Gull, Nasse, and Valentiner have been conspicuous as opponents of the theory of this form of paralysis. With the exception of the paper of Dr. Gull, we have had no opportunity of studying the arguments of these physiologists; but their reasoning must have had some weight, since it has been sufficient to cause Romberg, one of the promoters of the view that paraplegia may be due to a reflex action, to change his views and pass over to the side of its opponents. Thus deserted by one of his staunchest supporters, we conceive that Dr. Brown-Séquard has considered himself as thrown on the defensive, and has taken up the cudgels to defend a theory, to the elucidation of which he has devoted so much of his time. His arguments in favour of the existence of this affection we shall briefly review, although our space will not allow us to cite the cases which he quotes in support of his views.

Dr. Brown-Séquard defines reflex paraplegia as—

"A paralysis of the lower limbs, due to an excitation which has come to the spinal cord from a sensitive nerve. The excitation after having reached this nervous centre, may be reflected on the blood-vessels of this very centre, or on those of the motor nerves or the muscles."

As to the starting-point of the excitation, this appears from the numerous cases cited to be very various—the uterus, the bowels, the kidneys, the lungs, diphtheria, teething, affections of the bladder, prostate, or urethra, irritation of the nerves of the skin, &c.; but in all the paralysis was preceded by the irritation

presumed to be its cause, and the removal or mitigation of the latter was almost as invariably attended by an amelioration of the paralytic symptoms. These cases (not from the author's own note-book, but from a variety of independent sources), in our opinion, of themselves go far to prove his case. But were this all, the matter would simply resolve itself into the old squabble as to the distinction between *post hoc* and *propter hoc*, and it would be a sheer waste of time to discuss the subject here.

More proof, however, remains behind of a more purely scientific nature, and drawn from the pathological conditions upon which the affection depends. A reference to the definition above quoted will show that the author holds that the peripheric excitation after being transmitted to the nervous centre (the cord) may be reflected, 1st, on the blood-vessels of the centre, 2nd, on those of motor nerves, and 3rd, on those of the muscles, any one of which states may cause the paraplegia. The first, however, is that on which he seems to place the most reliance, as indicating the true pathology of reflex paraplegia; and that this change actually does occur he establishes, apparently beyond a doubt, by means of the following experiment, in which he appears to have repeatedly obtained the same results.

Having placed a tightened ligature on the hilus of the kidney or on the blood-vessels and nerves of the supra-renal capsules, *he has seen* contraction of the blood-vessels take place in the pia mater. However we might treat this statement, if emanating from an unpractised operator, we cannot but believe that it affords great confirmation to the views of our author, whose experience and powers of observation no one can deny.

Again, he has, assuming this pathological theory to be true, practically, and with the most beneficial results, applied it to the treatment of this affection. This he does in the following manner, and this in fact is the treatment which he recommends in all cases where we have convinced ourselves that the paraplegia is not centric in its origin. The treatment is directed, 1st, against the external (or exciting) cause; and, 2nd, against the paralysis itself. The following details are so concise and clear that we give them at length in the author's own words, as we would despair otherwise of putting them before our readers in a condensed, and at the same time, an intelligible form:—

"1. When it has been ascertained from what organ or from what nerve starts the nervous influence which causes a reflex paraplegia, besides the treatment that is appropriated to the nature of the local affection (of that organ or nerve), it is of the greatest importance to try to prevent or to diminish the transmission of any nervous influence from the diseased nerve or organ to the spinal cord. All the means usually employed to alleviate pain will be of service in such cases. If possible, we must try to paralyze for a time the sensitive nerves that convey the morbid influence to the spinal cord. Even a momentary suspension or

diminution of the transmission of this influence may be very useful. Narcotics ought to be employed in injections—in the bladder, if that organ be the place from which starts that morbid influence; in the vagina, if the uterus be the place; and in the rectum, if the large intestine be the place. Narcotics ought to be taken by the mouth if the stomach, the small intestine, or the kidneys are affected. In case of a pneumonia producing a reflex paralysis, inhalations of chloroform (which, by the way, have been successfully employed against the inflammation itself) may prove useful.

"2. The object of the means just proposed is to diminish the cause of the paraplegia; the object of the means we will now speak of is just the same, although it may seem to be quite different. Excitants or revulsives applied to the skin of the legs have been warmly recommended by Graves, who has obtained good results from their use. Probably the mode of action of these means consists in producing for a short time the same effect as the irritation which is the cause of the paralysis—i. e., a contraction of the blood-vessels of the spinal cord; but, according to a well-established law, if such a contraction becomes considerable, the muscular fibres are soon exhausted, and a relaxation of the contracted fibres takes place, and, as a consequence of this relaxation, a dilatation of the blood-vessels occurs. Of all the causes of irritation capable of producing a contraction of blood-vessels by a reflex action, none has more power than cold. In consequence of this fact, I think some of the modes of application of cold to the spine (modes of which we will speak by and by), ought to be employed in cases of reflex paraplegia. But the excitation in those cases must be very powerful, and able to produce a very considerable degree of contraction, so that the consequent exhaustion and dilatation may be obtained. With the same view, we may employ galvanism in the way we will soon describe.

"3. Another important principle, or rather another part of the same general principle of treatment, consists in making use of the following means to increase the quantity of blood in the spinal cord. Every night, and often in the course of the day, the patient should lie down on his back, placing his head, his arms, and his legs on high pillows, so as to produce by gravitation a congestion in the spinal cord. This simple means, which is also applicable in cases of hysterical paraplegia, and in almost all the cases in which there is an insufficient amount of blood in the spinal cord, is just the reverse of what should be done in cases of inflammation or congestion of the spinal cord or its membranes, or of disease of the spine, &c., in which cases the patient ought to lie flat on the abdomen or on one side of the body, and have his feet and hands on a much lower level than that of the spine.

"4. As regards the remedies to be taken by patients attacked with a reflex paraplegia, they must essentially be those which increase the amount of blood in the spinal cord, and augment the vital properties of this nervous centre, and also those remedies which render the blood richer in nutritive principles. We will soon indicate which are the best of these various remedies.

"5. As regards food and the hygienic rules, patients attacked with a reflex paraplegia must have the most substantial aliments, so as to improve the deficient nutrition of the spinal cord. They must take a great deal of exercise in the open air, and especially make use, as much as possible, of the paralyzed muscles."

There is a very important form of reflex paralysis which was prominently brought under the notice of the profession, as far back as 1833, by Mr. E. Stanley, in the *Medico-Chirurgical Transactions*. The conclusions of Mr. Stanley have been combated by Dr. W. Gull with considerable show of reason. He

objects altogether to the conclusions drawn by Mr. Stanley that the spinal cord was in a healthy condition, solely on the ground that he could not with the naked eye discover a lesion, and cites a case* which bears a close resemblance to some of those detailed by Mr. Stanley, where the microscope revealed extensive destruction of the substance of the spinal cord, a lesion which was quite invisible to the unaided eye. This, we freely admit, would have been a valid and conclusive objection to the theory, had subsequent examination proved that disease of the cord was generally, or even frequently, found in cases of so-called reflex paraplegia. But, so far from this being the case, Dr. Brown-Séquard has carefully examined this very point, and finds that in true reflex paraplegia no such lesion of the cord can be found but in exceptional cases. He quotes in confirmation of this, and with much apparent satisfaction, a case of urinary paraplegia as reported by Dr. Gull himself,† in which "hours were spent in the examination (microscopic), but with no other result than to show that there was no appreciable lesion of the cord, besides a slight and doubtful softness of the tissue in two points."

The main points of diagnosis between the various forms of paraplegia are carefully detailed, and the treatment of each shortly indicated. What pleases us perhaps most of all in the work, is its eminently practical tendency. Books of this kind, especially if involving controversy, are apt to be, and too often are, so choked up with mere detail, objections, answers to objections, &c., that the reader, with an honest wish to arrive at the truth, becomes bewildered; while he who looks for anything of practical value, anything which may help him in time of need, looks in vain. It would be, after all, a matter of comparatively trifling import to the physician to know how and where certain fibres decussate, through what portions of the cord sensitive impressions are transmitted, &c., if he were convinced that all this would never aid him to alleviate a pang or lengthen a life. But show him how he may by such study not only gain the means of doing good, but also acquire a knowledge of how the means of cure—drugs as well as other remedial measures—act on the system, and you at once command his earnest attention. Such a book is the one before us. The physician is not lost in the physiologist, but on the contrary the two sciences go hand in hand, as they ever should, in the pursuit of truth.

It is, of course, impossible for us to enter into anything like general details of treatment, but we will very briefly notice a few points connected with this branch of the subject which well merit

* *Medico-Chirurgical Transactions*, 1856, p. 196.

† *Guy's Hospital Reports*. Third Series. Vol. IV., Case 17, p. 176.
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the careful attention of every one who aspires to the position of a well-informed physician. The first point deserving of notice, and which, if we grant the existence of reflex paraplegia and the correctness of our author's pathological theory, is of paramount importance, is the careful distinction which must be made between reflex paraplegia, and those forms of paralysis which result from an inflammation or congestion of the spinal cord or its membranes.

In the one case there is a superfluity of blood in the spinal cord, in the other a deficiency, from which it will at once be evident that the means of treatment applicable to the one case would, if applied to the other, simply add fuel to the flame. And yet how many are content to go on blindly prescribing strychnine, belladonna, iodide of potassium, and the ergot of rye, although the action of these agents on the spinal cord are directly antagonistic. The principal medicines applicable to the diseases which are characterized by hyperæmia of the cord are stated by Dr. Brown-Séquard to be belladonna and the ergot of rye, while in the case of anæmia of the cord (reflex paraplegia and non-inflammatory softening) strychnine and iodide of potassium are the best adapted.

Another point of considerable interest in regard to treatment and to which the author appears to attach considerable importance, is the position which the patient is directed to assume in order to favour or prevent, as the case may require, the gravitation of blood to the cord. In reflex paraplegia he is made to lie on the back, with the limbs raised above the level of the body, while in a case of myelitis he is enjoined to lie on the side, or even to assume the prone position, with the limbs drooping to a lower level. Thus he shows us at every step, by impressing on the mind the mode of action of remedial measures, how fatal would be a faulty diagnosis.

With reference to the action of strychnine on the cord, there are some very interesting remarks, which will astonish those who have not kept in view the various opinions on this point which have agitated the scientific world :—

"It is generally believed that strychnine acts on the spinal cord as an excitant, *i. e.*, in the same way as the application of galvanism, of heat, of a caustic, or a mechanical irritation. This is a great error; strychnine is not able to produce the least excitation on the spinal cord. The experiments of Van Deen, of Dr. Marshall Hall, those I made ten years ago, alone, or with my pupil, Dr. Bonnefin, and those recently published by M. Martin-Magron, and M. Buisson, in their most important paper, "On the Comparative Action of Woorara and Strychnine,"* cannot leave the least doubt on this point. It is only in increasing the reflex faculty of the spinal cord that strychnine seems

* See *Journal de la Physiologie de l'Homme, &c.*, Nos. vii. and viii., 1859, and Nos. ix. and x., 1860.

to cause convulsions. This vital property of the cord reaches such a very high degree, that any external or internal excitation brings on a reflex tetanic contraction, the violence of which, according to a well-known law, is in proportion to the degree of the reflex faculty. So long as the spinal cord does not receive some kind of excitation, however powerfully poisoned by strychnine it may be, there is no convulsion. I have seen frogs, deprived of respiratory and voluntary movements (after the extirpation of the brain and the medulla oblongata), remaining hours, days, and even a week, without the least convulsion so long as they were not touched, although they were poisoned by a large dose of strychnine; but the least touch produced in them the most powerful reflex tetanic spasms."

To those who would study this interesting subject we would recommend a perusal of the series of able papers by M.M. Martin-Magron and Buisson, which are alluded to in the text. For ourselves, we confess that we are by no means convinced that the theory of Dr. Brown-Séquard is the correct one. If it is only by increasing the reflex faculty of the cord that the strychnine acts, we presume that every convulsive act requires a distal or peripheric excitation. Whence then is derived the excitation which causes the convulsions and tetanic spasms which invariably, in so far as our experience goes, takes place in an animal poisoned with strychnine? To this our author does not even allude, while M.M. Martin-Magron and Buisson, as the following extract will show, only mention it as a subject with regard to which they have no data to form a decided opinion. The following is the passage alluded to :—

"It is still an open question whether, as Kölliker appears to believe, the brain can *excite* the cord to produce convulsions, or whether the convulsions which follow the cerebral action are not the result of the rubbing of the skin against the parts which support the body during voluntary movements. We have not, up to the present time, made experiments sufficiently numerous to pronounce decidedly on the subject; but nevertheless we incline to the latter opinion."*

It is with regret that we pass over many points of interest which the space at our command does not permit even of our mentioning. A careful perusal of the work before us has induced us to form that high opinion of its merits which we have already expressed. A few typographical errors, a few faults in punctuation which render the meaning unnecessarily obscure, and perhaps a few trifling Gallicisms, are all that hypercriticism can find to note. The work is one which should be in the hands of every one who may be called upon to treat such disorders. Of all the works on the subject of which it treats which have come under our notice it is *facile princeps*, while upon its talented author it reflects the highest credit, and is in itself an eloquent commentary on the labour of twenty years.

* Op. Cit. tom. iii., p. 133. 1860.

VIII.—*The West India Quarterly Magazine.*
Nos. I. and II. Jamaica, 1861.

THIS magazine, which has just reached us, merits our notice as being something of a literary novelty. It is, we think, a somewhat bold idea to combine medicine, general science, and light reading in one and the same periodical; but we see no reason why it should not succeed, although we fear that it can never take a very high place in medical literature while contributors are cautioned that "no communication, the insertion of which would exclude the magazine from the drawing-room, or prevent its being placed in the hands even of the most sensitive, can for the future be received." From this we may assume that the medical element will be popularized to suit its readers, the great bulk of whom are non-medical; and this indeed is, in the circumstances, perhaps the best thing the editor could do. With such a regulation as the above, scientific medical literature cannot flourish; and as the regulation itself is perfectly necessary, we think the course we mention would be in all respects the best. Several of the articles are exceedingly interesting and well written, among which we may mention especially a paper by Mr. Hoffman on the manufacture of sugar, and another by the editor on the necessity for compulsory vaccination, in which this important subject is treated with considerable ability. The editor, Mr. Croskery, is a member of the medical profession, and we heartily wish him and his journal every success.

SELECTIONS FROM MEDICAL JOURNALS.

I.—A STATISTICAL CONTRIBUTION TO THE DIAGNOSIS OF
CANCER OF THE STOMACH.

BY JAMES C. ORTON, M.D., OF NEW JERSEY.

THE following paper is based upon an analysis of sixty recorded cases of cancer of the stomach, collected principally from periodical medical literature. These cases are generally not well reported, the writer having too exclusively a single idea in view, and paying too little attention to other but less prominent facts. In taking, however, the aggregate of cases, each has a relative importance, and one in a measure corrects the other.

Of all the internal organs of the body, with the single exception of the liver, the stomach is most frequently the seat of malignant disease. From reliable mortuary reports of continental cities, it appears that nearly one-fourth of all the fatal cases of cancer have their origin in the stomach. It is impossible,

from the imperfect manner in which registration is kept, to arrive at any other than the most general conclusion as to the accuracy of this statement in regard to the disease in this country. We may, however, in the most general manner, state that cancer of the stomach is more frequent in old luxurious cities, than where the style of living is comparatively simple. The period of life at which cancer is most likely to attack the stomach, is included between the sixtieth and seventieth years. It is next most frequent between the fortieth and fiftieth years. Under twenty-five I am not aware that it ever occurs as a primitive affection, while the upward limit cannot be defined.* Males are more subject to it than females, in the proportion of four to one, and married females to the unmarried, in the proportion of six to one. It is impossible to decide as to the influence of habits or occupation in the production of this disease. The intemperate rank about equally with the temperate,† while all the trades and professions are represented in about the proportion of the numbers who follow them respectively.

CARDIAC EXTREMITY.

Cancer occurs at the cardiac orifice of the stomach less frequently than at the pyloric, or in the body of the organ. Males are more liable to be the subjects of this situation of cancer than females in the ratio of nine to one.‡ It is eminently a disease of advanced life, the majority of cases occurring after the age of sixty, and but seldom previously to the age of fifty. No predisposition or habit of life seems to determine the location of cancer in this situation. The subjects of it are generally of advanced age, temperate, and engaged in healthful occupations. The exceptions to this rule are not of a kind to invalidate it. The duration of the disease rarely extends over several years from the occurrence even of the first symptoms. The great majority terminate fatally within one year, while about half do not live beyond six months.§ The symptoms of cancer of the cardiac extremity of the stomach vary with its seat, whether located within the orifice, primarily, or seated in the cardiac portion of the organ, and extending and involving the cardia secondarily. They vary also according to the nature of the cancerous growth, whether it be of the scirrhus or encephaloid variety. When cancer is situated at the cardiac orifice, the earliest symptom is dysphagia,|| or difficulty of swallowing food, attended with nausea, which soon amounts to vomiting of ingested matters. If the cancer is of the scirrhus variety, these symptoms may continue for a considerable time, and be intermittent in their severity. The dysphagia is a most distressing symptom, attended with severe pain at the pit of the stomach, and sometimes with paroxysms of threatened suffocation. The patient complains that his food does not enter his stomach, and that his stomach is closing up. He indicates very nearly the point of stoppage of his food at the cardiac orifice. These symptoms are diagnostic of the seat of the stricture. The pain which attends

* The analysis of 60 cases gives 26, 2; 29, 1; 30 to 40, 10; 40 to 50, 15; 50 to 60, 11; 60 to 70, 18; 70, 71, 72, 78, 82, each one.

† Of intemperate there are given 5; temperate, 6.

‡ Of 60 cases, 10 were at the cardiac orifice, 16 in the body, and 34 at the pylorus. Louis collected 13 cases which gave, cardia, 1; body, 5; and pylorus, 7. Bouillaud gives 8—cardia, 1; body, 2; pylorus, 5. Of 10 cases, 9 were males, 1 female. Of 10 cases, 1 was between 30 and 40; 1, 40 and 50; 2, 50 and 60; 5, 60 and 70; 1, 72.

§ Of ten cases, 2 lived 3 months; 2, 6 months; 1, 8 months; 2, 1 year; 1, 2 years; 2, several years. Of 10 cases, 6 were scirrhus; 2, encephaloid; and 1 mixed; 1, doubtful.

|| In 7 cases, limited to cardiac orifices, 5 had dysphagia as the first well-marked symptom.

efforts at deglutition is caused by the arrest of the bolus at the strictured point, and extends to the back, shoulder-blades, and hypochondria. In these situations it is of a dull, aching kind, while at the cardia, it is often extremely violent and lancinating, and only relieved by the rejection of the food, or its passage beyond the stricture into the stomach. The pain is, however, by no means constant in its character and seat, depending probably much upon the degree of constriction, and occasionally upon the nerves implicated.* It may be so constant as to stimulate gastritis, or it may be so violent as to resemble angina, and lead to attacks of convulsive diseases. In this manner cancer of the cardia has been treated for hepatitis, gastritis, colic, angina, and chorea. Undoubtedly, in this latter case, the par vagum was involved, as on dissection it has been found in other cases. Pain, as a diagnostic symptom, must be estimated by its seat and relation to the dysphagia. When located about the ensiform cartilage, fixed, unvarying, and lancinating, or of a dull, aching character during the efforts of deglutition, it becomes of considerable importance. In itself considered, however, it has no diagnostic value.

Vomiting is an early symptom,† generally immediately succeeding the appearance of dysphagia. It seldom, if ever, precedes this latter symptom in cancer of the orifice alone.‡ The peculiarity of the vomiting is, that it promptly follows the ingestion of food, and on this account is of great value in diagnosis. This vomiting is noticed in some instances as being easy, free from retching and pain, or it may be very painful. It is intermittent in severity, often being absent for several days or even weeks together. In these cases the disease is very slow in its development and progress, extending over several years, or it progresses very rapidly, and is of the encephaloid variety, the freedom from dysphagia and vomiting being due to the ulceration of the cancerous growth, and consequent enlargement of the orifice. Two peculiarities, almost without exception, mark the vomiting in the latter stages of the disease. More frequently the progressive constriction of the orifice, by the growth of the cancerous mass, finally amounts to a complete arrest of solid food in its passage to the stomach, and a consequent regurgitation of the ingesta in an unaltered state.§ Less often vomiting of blood occurs, which marks the ulcerative stage of cancer, and generally the encephaloid variety. In either case, taken in connection with other symptoms, it gives unmistakable evidence of the nature, seat, and stage of the disease. Allied to vomiting is a discharge of ropy mucus from the mouth, which is sometimes constantly present. This discharge is often sour and very offensive.

The appearance of a defined tumour in the progress of the case, marks the character of the growth as encephaloid. This is due to the greater extent of this form of cancer, and its tendency to involve greater surface by encroaching upon the stomach itself. The tumor is located at the pit of the stomach or under the ensiform cartilage, and is painful on pressure, and occasionally pulsates.|| Its location at the ensiform cartilage, taken with other symptoms, gives

* In one case there was intense pain in the chest, which subsided, but no pain in stomach. This returned without relief, and ended in attacks of chorea every seven days; no vomiting or symptoms about stomach, except appearance of a tumour.—*Med.-Chir. Rev.*, July, 1837.

† Of 10 cases, 9 had vomiting as an early symptom; 1, no vomiting at all.

‡ Of 7 cases, 6 had dysphagia and vomiting; 1 not mentioned. In one case it lasted several years.

§ Of 10 cases, 5 had regurgitation of the food near termination of disease; 2, vomiting of blood; 2, simple vomiting, of whom 1 died of dysentery, the other suddenly; 1, no vomiting, but had chorea, &c.

|| In 4 cases tumour was felt; 3, encephaloid and extended into stomach; 1, doubtful; 1, scirrhus, but just discernible.

it great value in accurate diagnosis. If but a slight degree of hardness is felt, it is scirrhus probably.

Of the symptoms more remote from the stomach, but consequent upon the disease existing within it, the earliest is constipation. This is due to the absence of solid food. In one case, however, diarrhœa with bloody discharges ushered in the disease. This may be easily explained by the fact that the cancer was encephaloid, and involved a considerable portion of the cardiac extremity. But when the disease involves principally or primarily the orifice, admitting only the introduction of liquids, constipation is the natural consequence. Later in the history of the case, diarrhœa supervenes, owing to the irritation of the cancerous debris resulting from ulceration, and dark-coloured offensive stools are discharged.

As a necessary result also of this difficulty of introducing substances into the stomach, emaciation commences early, and gradually progresses, until in some instances, where the sufferer dies of inanition, it reaches an extreme degree.*

The emaciation, of course, has a direct ratio to the degree of dysphagia which exists. In extreme cases it has become impossible even to swallow liquids. The dysphagia again has a direct relation to the condition of the morbid growth, whether it be scirrhus or encephaloid, or whether it entirely or only partially surrounds the orifice. If the stricture be encephaloid, it forms a tumour which will early ulcerate, and render the orifice again patulous, and before emaciation has far advanced, food may again be introduced into the stomach. But if the stricture be purely scirrhus, as it far more often is, it will generally surround the orifice, and gradually but progressively contract its calibre, until it prevents the passage even of liquids, unless relieved by ulceration. It is fair to presume, therefore, that those cases attended with the greatest degree of emaciation, are of the encephaloid variety.

The general symptoms of this form of cancer are not striking. The appetite is often voracious, owing to the obstacle to taking food; the tongue is often coated, but this is due to the state of the bowels suffering constipation; the countenance is never characterized by the sallowness of cancer.†

BODY OF THE STOMACH.

As anatomically, so also pathologically the body of the stomach occupies a position intermediate to that of the orifices, in respect to frequency as the seat of cancer. The period of life at which it generally occurs is about the age of sixty.‡ Males are four times more subject to it than females.§ To no occupation, habit of life, or peculiar temperament can the attack be traced.

Every portion of the body does not seem to be equally liable to cancer. First, in order of frequency, is the larger curvature with the great cul-de-sac; second, the body generally; lastly, the smaller curvature.

We have shown that cancer, occupying primitively and only the cardiac orifice, is without exception scirrhus. Cancer of the body, on the other hand, is always encephaloid or colloid, with one exception, and that is when it is primarily seated in the smaller curve.||

The duration of this disease is variable, but generally extends over several years. Often dyspeptic symptoms are complained of for upwards of twenty

* Of 10 cases, 7 were emaciated, 3 not greatly.

† Of 10 cases, one note is made of the countenance, and that is that it was very dark.

‡ 29, 36; 40 to 50, 4; 50 to 60, 4; 60 to 63, 5 (82).

§ Of 16 cases, 12 were males, 4 females.

|| Of 6 cases in large curve all were encephaloid; 3 in small; all were scirrhus.

years, when the severe symptoms set in, and the case terminates fatally within a few months from this attack. The shortest period given was two months.*

The symptoms vary with the seat and character of the growth, whether it be in the greater or smaller curvature, and these localities determine for the most part whether it is scirrhus or encephaloid.

The earliest symptoms which arise from cancer of the body of the stomach, seated in the larger curve, cannot be distinguished from those of ordinary cases of dyspepsia. These do not, however, continue long previously to the onset of a new and marked series of symptoms which are peculiar to the malignant disease. Patients usually acknowledge, on being questioned, that they have long suffered from heartburn, pyrosis, &c., but without loss of general health. But when the distinctive symptoms of cancer become manifest, the features of the case materially change.

The constant symptom of all gastric derangements is always present in cancer of the larger curvature, but is too variable in constancy and severity to be considered specially. Generally it amounts to a mere uneasiness in the early stages of the disease, gradually increases in severity, being most troublesome during the digestion of food, and relieved by vomiting. It finally becomes a seated pain at the pit of the stomach, aggravated after taking food, or on pressure being made over the epigastrium. Rarely the stomach is not the seat of pain.† In such cases it may be complained of only in the lumbar region, or chest; more often it has taken the form of a "colic," easily allayed by carminatives. Vomiting is the symptom of which patients more usually complain.‡ It may be one of the earliest symptoms, and towards the termination of the case become much less severe; but more often these relations are reversed, the vomiting being later, and occasionally causing a sudden termination of the disease. It is very rarely absent during the whole course of the complaint. The only peculiarity of the vomiting in cancer of the large curvature, in its earliest stages, is the period after eating at which it occurs. This is according to the situation of the growth or ulcerated surface, whether it is near the cardiac or pyloric orifice. In proportion as it recedes from the former, is the length of time at which emesis occurs after taking food.§ When the disease is located in the left extremity the pain is often very severe, almost immediately after the act of deglutition, but the vomiting is not constant. When the centre of the large curve is alone involved, pain sets in soon after eating, and gradually increases until vomiting takes place, when it begins to decline. This occurs from half an hour to two hours after taking food, but if the disease involve principally the pyloric extremity, the vomiting is delayed three or four hours. Vomiting becomes diagnostic of cancer, when the matters ejected are offensive and contain blood. This is the "coffee-grounds" appearance which marks the ulcerated cancer, and, of course, the last stage of the disease. In reviewing this system it may be said that vomiting is not invariably present, is milder and less frequent when the disease is at the left than at the right, and when in the centre may be entirely absent. It is often preceded and followed by offensive eructations in the early stages, and discharges from the mouth of thin, ropy mucus.

In cancer of the larger curvature of the stomach, a tumour, or fulness, or unnatural hardness may be discovered at some period of the disease in the

* Of 16, 5 suffered 2 years; 2, 1 year; 1, 6; 1, 8; 1, 2 months; two slightly dyspeptic, many years; 1 died 4 months after severe symptoms.

† Of 3 cases of encephaloid tumour of the great cul-de-sac, 1 had great pain after eating; 2, severe pain in stomach.

‡ Of 10 cases, 8 vomited; 1, not; 1, no note.

§ Of 3, 1 vomited early in disease; none towards close; 1 vomited at close blood, pus, and bile; 1, not given.

epigastrium. Taken in connection with the symptoms which have preceded or which accompany it, this tumour may furnish the first unequivocal evidence of the precise nature of the difficulty. The period at which this tumour may be discovered on examination is doubtless early, although it is generally not detected until the disease has far advanced. When the cancerous growth is seated in the left extremity, the tumour may be found just below the ensiform cartilage, in the same situation as when the cardiac orifice is involved with cancer of the cardiac extremity of the stomach. When the tumour is large it is sometimes movable, which may assist in distinguishing it from those cases where the orifice is involved. It more often is felt much lower, and has been mistaken for a tumour of the spleen and pancreas. In cancer of the central portion of the curvature, the tumour is felt near the umbilicus, and most generally above and to the right. Generally a distinct well-defined tumour may be made out, but in other cases, where the disease is extensive, involving all the coats, it may be felt only as an undefined hardness. When pressure is made upon this tumour, the pain or uneasiness is greatly increased, and sometimes it gives rise to vomiting. A peculiarity several times noticed is the subsidence of the tumour after free purgation, probably from a breaking down of its substance. The tumour has also been observed to pulsate, when the disease was situated near the pylorus. In such cases it has been found situated directly over the aorta, of the pulsations of which it partook. It has only a pulsatory movement, not the lateral expansion of an aneurism, for which it might be taken. It does not increase to a great size, but generally remains apparent only on manipulation.

The general symptoms which may arise are various, and by no means uniform. The bowels are constipated early, but later often free, with tendency to diarrhoea. The appetite is always poor when the disease involves the cardiac extremity, but more often voracious and capricious when seated in the central position. The tongue is not much changed; the pulse and general strength proportionate to the ability to take and retain food. Emaciation is far from being marked in most cases, though it is progressive. The complexion but seldom, comparatively, has any distinctive or diagnostic appearance. The immediate cause of death is sometimes sudden hæmorrhage from the stomach; more often the result of frequently-repeated attacks. Œdema often comes on, and ascites, due to the impoverished and cachectic condition of the system. Cancer of the smaller curvature of the stomach runs a course somewhat different from that which we have first considered. This is due to its anatomical position and pathological structure. Cancer in this situation is less subject to the pressure and irritation of food, to the compression exercised by the stomach in its contractions, and, finally, it is always of the scirrhus variety. Pain is generally present, and often is extreme and constant, but vomiting is rarely a symptom. Occasionally, however, it occurs after ulceration has taken place, and sometimes it proves fatal at once from the quantity of blood lost. When it is present, it comes on some time after eating. A tumour is less frequently felt in this situation than when the larger curvature is the seat of difficulty.

PYLORIC EXTREMITY.

The pylorus is nearly twice as frequently the seat of cancer as both the cardiac orifice and the body. It occurs five times as often in males as females. The age most subject to it is included between the fortieth and fiftieth years, it being a disease rather of middle than very advanced life, as in the case when the cardiac orifice is involved.*

* Males, 30; females, 6: 26, 2; 30, 4; 40, 11; 50, 7; 60, 8; 70, 2; 78, 1.

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The duration of the disease, dating from the appearance of acute symptoms, does not often extend over one year, while a majority prove fatal within six months. Symptoms of dyspepsia are experienced for many years; but it is not probable that they bear any causative relation to the disease. As in the other localities noticed, cancer of the pylorus does not depend upon any particular habit, or occupation, or condition of body traceable. The great preponderance of males over females might lead to the supposition that habits of intemperance or occupation might have, if not a predisposing, an exciting cause in its production; but facts do not confirm the opinion.

Pain is again a prominent symptom in most cases. It varies with the action of the stomach, whether quiet or engaged in digestion. It is least, and often entirely absent, in the interval of taking food. Pressure over the pylorus greatly increases it, as in the former case, when pressure was made over the tumour. It may be entirely absent; and in such cases, the disease runs a very obscure course.

Vomiting is rarely absent. It generally takes place several hours after eating, and is followed by great relief. The effort is not great, and the act is desired for the relief which it gives. Vomiting, in cancer of the pylorus, is not wholly dependent upon the contraction of the orifice, as that condition occasionally exists when vomiting is a persistent system. There is nothing diagnostic in the character of the vomiting until ulceration takes place, when blood and the cancerous débris are the matters ejected.

As, with careful manipulation, the pylorus may be felt through the walls of the abdomen in a healthy person, not unusually fleshy; so the detection of a tumour, or the early enlargement and induration of the cancerous growth seated in this place, may be detected. On the care with which the early examination is made depends the early detection of the disease. The symptoms already referred to are not individually or collectively reliable in the formation of an opinion; but in connection with the detection of a tumour they become of great importance. The tumour is generally seated to the right of the umbilicus and ensiform cartilage, at a variable distance from them, both laterally and vertically. It increases the pain or distress which may have previously existed at that point, to make pressure upon it directly or even remotely. The tumour may, however, be absent or undefined, in which case it protrudes backwardly. If other symptoms are then not marked, the disease passes on to a fatal termination unrecognized.

The general symptoms are such as belong to cancer of the cardiac orifice. Obstinate constipation is early present, but as the case advances diarrhoea alternates. Emaciation is generally marked, but depending, as in the other case, upon the degree of ulceration. If this should be such as to leave the orifice free, vomiting will not be so frequent, and the food may have time to digest and the fatty matters to pass into the intestines and be absorbed. The complexion is not more markedly cancerous than in other instances. Death takes place usually from exhaustion, but not from inanition, as in cancer of the cardiac orifice. The exhaustion is occasioned by the constant suffering, and frequent losses of blood by vomiting.

In reviewing briefly the symptoms of cancer of the stomach when located at the cardiac and pyloric orifices, and in the body, we cannot fail to notice some points of difference worthy of attention in diagnosis. Pain is common to all, and not reliable in itself. Taken in connection with other symptoms it has a relative value. Vomiting is also common to all, but peculiarly different in cardiac cancer from that occurring in the other situations. It follows during or soon after the act of deglutition, which is painful. In cancer of the body or pylorus it takes place at a period after eating, depending upon the situation of the disease from the cardiac orifice. A tumour is discoverable only in cancer of the body and pylorus primitively. It is not possible with this alone to dis-

criminate between cancer of the pylorus and body, so variable is its location. In general we may say that it is larger, and more easily defined as a tumour, when the body is the seat of a cancerous growth.

CONCLUSIONS.

The most important symptom, therefore, in cancer of the cardiac orifice, is *difficult deglutition*, the constriction being seated on a line corresponding with the orifice.

The most reliable symptom of cancer of the body is a tumour which cannot ordinarily be early detected. Of the earliest symptoms, pain always after eating, finally relieved by vomiting; eructation of fetid gas; ropy, and fetid mucus; intervals of entire relief; absence of fever; constipated bowels—are most reliable.

In cancer of pylorus, the tumour early detected, taken with the seat of pain, which may be lancinating, relieved by vomiting, with the other symptoms, as in case of cancer of the body, give the best evidence of the nature of the difficulty.

In the diagnosis of cancer from other affections of the stomach, the symptoms now reviewed would have to be relied upon. The difference is ordinarily well marked by the occurrence of several of these symptoms together.—*American Medical Times*.

II.—SUBSTITUTION OF A BLUNT INSTRUMENT FOR A SHARP BLADE IN THE OPERATION FOR HERNIA.

A long time since, in the record of Professor Malgaigne's lectures, we stated that this surgeon uses more frequently a probe or spatula at the conclusion of the operation for hernia than the knife. This procedure affords the greatest chances of averting hæmorrhage, and recently, Mr. Amédée Joux of La Ferté-Gaucher described, in a paper inserted in the *Journal des Connaissances Médicales*, the advantages he has derived for twenty years from *laceration* as a substitute for *incision* in the surgical treatment of all varieties of incarcerated hernia. He concludes his memoir with the following remarks:—

The operation for strangulated hernia is not so perilous as we find it stated in classical works on the subject, and to avert the dangers authors dwell upon, the operation should be resorted to at an early period, and the parts should not be fatigued by often useless and always hazardous attempts at reduction.

The skin only should be divided with the knife; the remainder of the operation should be performed with a blunt instrument, such as a spatula or a probe, but always by *laceration*.

As to the division of the stricture, in order to avoid hæmorrhage and to avert the disadvantages attendant on the use of the blunt-pointed bistoury, which often divides the structures in a greater extent than the surgeon contemplates, it is in all cases proper, after having introduced the extremity of the finger into the stricture, to insert the small end of the spatula, its rough surface being in contact with the constriction, and to use it as a lever the fulcrum of which is afforded by the finger, and the power is applied to the broad extremity of the instrument; by thus endeavouring to distend the tightest parts of the ring, dilatation is very speedily effected and reduction may be obtained, if indeed it does not take place spontaneously, *without the escape of a single drop of blood*.—*Journal of Practical Medicine and Surgery*.

III.—SUCCESSFUL OPERATION FOR INGUINAL HERNIA WITH THE TESTICLE AND SAC BOTH ABOVE POUPART'S LIGAMENT. By F. TAYLOR BRADFORD, M.D., of Augusta, Ky.

The unusual character of the present case, together with the mortality attending the operation for hernia, renders the discussion of the subject, as well as each individual case, a matter of peculiar interest.

Whilst *Amussat* and his long list of followers insist upon "subjecting the hernial tumour to continued pressure and kneading for *days* together," *Desault* and his converts teach us to "think favourably of a hernia which *has not been handled* before the operation."

"Has the practice of opening the sac had any influence on the great number of unfavourable results?"

Petit, Le Dran, Munro, Sir Charles Bell, Bransby Cooper, Aston Key, Luke, and Gay, say yes; *Dupuytren, Richter, Hey, Heister, Sir A. Cooper, Lawrence, South, and Hancock*, say no!

We are continually drifting to extremes, and it is only by the "*second sober thought*" that we may move steadily by the legitimate landmarks of safety.

The fact that a solitary instance here and there has yielded to the oft-repeated efforts by purgation, the taxis, and a long delay for the hopeful chances of nature, is not of itself *prima facie* evidence that the rule is founded in conservative surgery.

Those who have read the very excellent work of Mr. Hancock cannot come to any other reasonable conclusion than that the great fatality of hernia is owing, not so much to the operation, or the manner in which it is performed by good surgeons, as to the delay incident upon continued and repeated efforts by the taxis, purgation, and rough and repeated manipulation of the part. If my limited experience is worth anything in matters of this sort, it is this: Place the patient under the influence of chloroform, embracing as it does the advantages of venesection, tobacco, the warm bath, tartar emetic, opium, &c., &c.; then make an effort by the taxis, and if you do not succeed, the indications are pretty clear that you have neglected the proper position of the patient, the best means of reducing, or that an operation should be advised or performed. My only object in making these remarks is to guard the practitioner against a *common error* that days must be spent in efforts by purgation, taxis, &c., &c., until the reasonable hopes for a timely operation are gone. They are not made, however, with a view to the present case. It was in the hands of intelligent men, and from this fact, perhaps, more than to the manner of the operation, does its success depend.

The subject of the present interesting case was a negro man, aged 45, of good constitution, whose muscular development was most extraordinary; his remarkable feats of strength and muscular prowess rivalling perhaps anything of the kind in the State. He had a wife and several children. A few days previously he was shaving a *mail* with a draw-knife, having one end against his belly; suddenly he complained of *colic pain*. I found him with swollen abdomen; sense of tightness across the lower part of the bowels; frequent desire to go to stool; inability to evacuate; occasional vomiting; hiccup; countenance anxious; pulse small, hard, and wiry. Immediately above *Poupart's ligament* was a large swelling, tender to the touch, slightly elastic, with a glossy cast as though it was oiled. The swelling was *peculiar*, and lay across the bowels, not perpendicular, measuring perhaps five inches in length. Upon examination of the *scrotum* only one *testicle* was found. When interrogated as to this state of affairs, he stated, "*he never had had but one.*" He then stated that twelve years before he had suffered in the same way, and after a day or two a physician had got his bowels acted on, and said he had reduced the bowel. But from that time

up to the period of this attack "he had always had a lump there; it never had gone away."

Drs. Duke and Sharp of Maysville, and Taylor and Cartwell of Washington, were in attendance; they had used the most efficient and prudent means to reduce the supposed strangulated bowel without effect. The known ability of these gentlemen, and the time which had elapsed, deterred me from further irritation of the part; but believing that the *testicle* lay above Poupart's ligament, and that inflammation of that organ at that point might give rise to a similar train of symptoms, and somewhat influenced by his former attack, I suggested the propriety of making a further attempt to move the bowels and delaying the operation a short time. It was cheerfully agreed to. The present condition and history of the case was the history of strangulation, *critically considered*, yet the former history of his attack, where the bowels had acted, influenced us to this conservative hope. The bowels, however, not being acted upon at the expiration of the time agreed upon, the patient was placed under the influence of chloroform, and the operation commenced. Taking hold of the integuments immediately over the tumour with the left forefinger and thumb, lifting them up slightly and guiding them up and down so as to be sure they were freed from the sac, I thrust the knife through the integuments with the back next the tumour, and cut directly upwards and outwards. This brought me near the sac, and of the remaining membranes (*intercolumnar fascia*, &c.) a little bit of each was pinched up by the forceps and cut into. A grooved director was then placed in the small *niche*, and each membrane carefully divided until the sac was reached. Its bluish transparent coat was divided like the former coats. The left forefinger was then passed into the neck of the sac; the stricture being found, the hernia knife (Le Gros Clark's) was passed up flat on the finger through the stricture, the edge then turned up, and the division made by a slightly *undulating* motion *directly* upwards—knife parallel to the *linea alba*.

The stricture was remarkably firm and tendinous, and was distinctly heard when divided by the knife. The sac contained a small quantity of limpid serum. A small portion of the bowel was of a deep purple hue, interspersed with spots, and a *knuckle* of an ash hue which gave rise to some uneasiness. The bowel being returned into the abdomen brought to view the *fugitive testicle* lying immediately under the sac. It was much smaller in size than the other testicle, but perfectly healthy in colour, and firmly adhered to its strange locality. I did not attempt to remove it. The wound was dressed in the usual way, and the patient recovered without much trouble.

Dr. Taylor and Dr. Cartwell are entitled to much credit for their faithful attention after the operation.—*American Journal of Medical Science*, Oct., 1861.

IV.—MORTALITY OF NEW-BORN CHILDREN.

ACADEMY OF SCIENCES.—A paper was read by M. Bouchut on the mortality of new-born infants.

After having reviewed the causes of this mortality, the author concludes by the following propositions:

The mortality of infants in general, taken in different social conditions, is at present in France *one sixth* for the first year, whereas it was formerly *one fourth*.

At the same age, the mortality is *one fifth* in boys, and *one sixth* only in girls. The mortality of children is more considerable in poor families than among the rich.

Cold increases the mortality of new-born infants, and in winter it is dangerous to take out children to the *mairie* (for the purpose of registration of birth) or to the church.

The mortality of natural, or legitimate children forsaken by their parents and brought up by hand in the country, is 11 per cent. in the first ten days of life, and 35 per cent. in the first year,

Foundlings brought up by hand with the feeding-bottle or otherwise, die in considerably larger numbers than those intrusted to wet-nurses.

The mortality of children of the middling classes, put out to nurse by the Administration, is 29 per cent. during the first year.

The mortality during the first year is more considerable in the 13 departments which surround Paris, than in each of the other departments of France, probably on account of the greater number of foundlings, of the absence of necessary care, and of the endemic or epidemic diseases of the capital, which extend to the adjacent departments.—*Journal of Practical Medicine and Surgery.*

V.—QUININE AS A PARTURIFACIENT.

The *Cincinnati Lancet and Observer* for July contains the proceedings of the Union Medical Society at Knightstown, Ind. Dr. Cochran reported an obstetrical case, in which the patient was exhausted, and the pains had ceased for some time. He gave ten grains of quinine at one dose, which was speedily followed by strong pains, and the birth of the child. It should be observed that the parturient properties of quinine have several times been under discussion previously before this society. We abstract an opinion or two of other members elicited in the discussion. Dr. Canada said, "It could not always be relied on; but he viewed it as the most reliable parturient remedy we have." Dr. Cronse thought that "when quinine was given in proper doses, it rarely disappoints."

On a former occasion, Dr. John Lewis said, "When I find a patient in labour, with a rigid os uteri, a tense pulse, and dry skin, I always give quinine freely, use dry cups over the sacrum, and the warm foot-bath, and expect the os uteri to relax, the pulse and skin to soften, the uterine contractions to increase in frequency and force, as surely and as certainly as I would nausea to follow the exhibition of ipecacuanha, or purging from jalap."

Bearing upon this subject, we may mention that Dr. J. S. Rich of Georgia, in the *Charleston Medical Journal and Review* for March, 1860, reports several cases of uterine hæmorrhage of alarming character that were speedily relieved by the use of quinine, after the failure of all other known means. Professor Edward Warren, now of Baltimore, says, "We have found nothing more likely to produce abortion in pregnancy than the administration of large doses of quinine." Several others have ascribed to it abortive powers, when administered in full doses. It is quite possible that it may be found to be a valuable parturient remedy, though in any ordinary doses we do not believe, as an abortive agent, it is at all to be feared.—*Med. and Surg. Reporter.*

VI.—FERRI CARBONAS EFFERVESCENS—A NEW FORM OF CHALYBEATE.

Dr. T. Skinner recommends the following formula for preparing effervescent carbonate of iron:—

R.	Acidi tart.,	℥ iij.
	Sodæ bicarbonatis,	℥ v.
	Ferri sulph.,	℥ x.
	Pulv. sacchari,	℥ j.
	Acidi citrici,	℥ ij.

1. Mix the sulphate of iron with the sugar and part of the tartaric acid. 2. Mix the citric acid with the remainder of the tartaric acid and the bicarbonate of soda. 3. Add the mixtures, and thoroughly incorporate them by sifting. 4. The whole is now to be thrown into a metallic pan set in a water-bath; in a few minutes it will separate, when it should be rapidly stirred until granules are formed. If preferred, it may then be flavoured with oil of lemon; hitherto, however, the preparation has been without it.

When the above is carefully prepared, it has all the appearance of the popular and well-known granular effervescent citrate of magnesia, with the addition of a slight yellowish-green tint. Every drachm and a half contains ten grains of sulphate of iron, which, with a complement of bicarbonate of soda, is certain to produce, in a state of solution, four grains of nascent protocarbonate of iron.—*Dublin Medical Press.*

VII.—CHLORIDE OF ZINC IN DISEASES OF THE SKIN.

Since Hanke in 1841 called attention to this remedy, Dr. Veiel of Cannstatt has employed the same in the following three forms:—

1. Spirit of alcoholic solution; equal parts of the chloride and alcohol.
2. Liquor or aqueous solution; chloride of zinc and muriatic acid, of each ten parts; water, five hundred parts.
3. Sticks or pencils, prepared like the sticks of caustic potassa, with which this form also corresponds in the manner of application.

The sticks are used to destroy hypertrophic lupus by repeated boring; in exfoliative and exulcerative lupus the application of the spirit, followed by the liquor, prove sufficient. Superficial or erythematus lupus requires the spirit attenuated by the liquor. Other cutaneous diseases benefited by these preparations are—obstinate eczema of the eyelids, lips, genitals, anus—the spirit painted over; solar and impetiginous eczema—the liquor applied daily; eczema of the tongue, fissures of the nipples, scrotum, or hands, callosities, &c.—one part of the spirit mixed with ten parts of the liquor; remains of psoriasis—spirit; a certain form of palmar psoriasis, with corn-like painful protuberances—stick; sycosis, favus, and some varieties of acne—liquor; circumscribed indurations on the nose, cheeks, and lips—spirit; chronic ulcers of the feet, with callous edges—spirit; cyst, ulcerating glands, fistulæ—spirit; condyloma, mollusum, seborrhoea, burns, chilblains—liquor.

The chloride of zinc enters into combinations with nearly all the elements it meets, and produces a descending irritation, which leads to contraction of the surrounding parts. Hence result diminution of the wound, speedy formation of pus, detachment of the crust and granulation, and finally a good scar. On this account the chloride is preferable to acids, caustic potassa, nitrate of silver, iodine, and other caustics.—*Zeitschrift d. Gesell. d. Aerzte zu Wien.*

VIII.—UTILITY OF BRAN IN THE MANUFACTURE OF BREAD.

The authorities judiciously attach much importance to the details of the manufacture of bread, and on the other hand, the trade endeavours to avail itself of certain scientific data to supply the therapist with hygienic and medicated bread. *Panification* is a question of paramount interest; and we can readily conceive why a surgeon holding an eminent position in the navy, Professor

Fonssagrives, has assigned to the subject a prominent place in his excellent treatise on elementary hygiene, which has just been published by J. B. Raillière and Sons.*

"Wholesome bread," say the Instructions of the Military Board of Health, "must be well leavened, i. e. present numerous holes in all its parts; it must exhale its own peculiar agreeable odour, the crumb must be homogeneous, elastic, and the holes must reappear after slight pressure of the crumb; in short, the crust must be yellow, sonorous, and everywhere adhere to the crumb. Bread is of bad quality, badly prepared or baked, when its smell is faint or mouldy; when its colour is too dark or unequal; when it contains lumps of flour; when the crumb rolls up into compact masses, which do not resume their shape when pressed, or is diffuent and greasy; finally, when the crust is pale, soft, or burned, and detached from the crumb." To be well digested, bread should be eaten fifteen or twenty hours after it has been baked. The desire of purifying to its utmost limits flour intended for bread, has induced the directors of meal-stores to separate from it principles, the assimilation of which was turned to good account. Bran, for instance, performs in the digestion of bread a part the importance of which, acknowledged in our days by American practitioners, and among others by Warren, had not escaped Hippocrates: "Brown bread," says he, "is siccative and aperient; white bread feeds more, and is less laxative." This is so true, adds Mr. Fonssagrives, that the question has been asked in latter times and not without some reason, if the very great frequency of constipation does not originate in the bread being made of flour too finely sifted. It is, however, a positive fact that a coarser bread, made of a mixture of various flours or rather rye-bread, very efficaciously removes torpidity of the bowels.

Mr. Fonssagrives considers as justly renowned in this respect the rye-bread of Ponscoff in Brittany, the taste of which is very savoury. The convalescent whose digestions are indolent may advantageously resort to its use; but in cases of habitual and stubborn constipation, this article of food is insufficient, and bran bread must be used.

"This bread," says Mr. Payen, "is much used in England. It is prepared with wheat, containing from 5 to 10 per cent. of bran. Its crust is dark-coloured, and its crumb is brown. Persons who use it take it but once or twice a week and ascribe to it cooling properties due to the indigestible part of the bran, which, in this case, acts mechanically in the same manner as white mustard seed."

Mr. Fonssagrives has frequently recourse in his own practice to bread made with equal parts of fine bran and wheat flour, and if the coarseness of its appearance does not deter them from its use, invalids derive much advantage from it in case of obstinate costiveness. A piece of bread weighing 3 or 4 ounces is eaten at each meal, and the latter is completed with ordinary bread. Dr. Lebarillier of Bordeaux, has also adduced cases in which almost unconquerable constipation was removed by the persevering use of bran bread. It is specially adapted to cases of intestinal torpidity connected with uterine disease; but its success, according to Mr. Fonssagrives, is frequently compromised by the carelessness of bakers, who refuse to prepare this bread by kneading it separately, a condition indispensable to its success: for bran bread, prepared simply by the incorporation of bran in a piece of kneaded dough, has an unpleasant aspect and no longer gives the results required.—*Journal of Practical Medicine and Surgery.*

* *Hygiène alimentaire des malades, des convalescents, et des valétudinaires, ou du régime envisagé comme moyen thérapeutique.*

IX.—ON PUS-CELLS IN THE AIR AND THE AEROSCOPE.

By Dr. EISELT.

During an epidemic of purulent ophthalmia, which occurred at the Foundling Hospital, near Prague, Dr. Eiselt had the opportunity of proving in his own person that infection may take place in other ways than by contact. As the attending physician, he took every precaution to protect his own eyes from any contact with the matter proceeding from the children, which it was easy for him to do, inasmuch as the syringing and cleansing of their eyes was performed by the sisters of the establishment. Still, being engaged in the hospital for several hours daily, he perceived smarting and heaviness of the eyes, followed afterwards by reddening of the conjunctiva, with an oedematous state of the portion lining the eyelids, and a considerable secretion therefrom. The same symptoms were observed in all the persons who had care of the children. Some of the nurses became seriously affected from getting matter into the eyes, and others they knew not how. The inconvenience, as regards the author, was checked by the use of weak stimulant collyria.

He asks, How comes it that acute purulent ophthalmia may thus be excited without any contact, in the common sense of the word? and refers in explanation to Pouchet's recent experiments with the aeroscope as described in the *Comptes-Rendus* for 1860. Professor Purkinje constructed a similar instrument for his use. Its principle consists in forcing a determinate quantity of the air to be examined over a glass-plate smeared with glycerine, which detains the particles of dust and microscopic structures for examination. By means of this instrument (for details of the structure of which we must refer to the paper) the air was examined which existed in a ward containing thirty-three children suffering from acute purulent ophthalmia, accompanied by abundant secretion. Pus corpuscles were at once detected in the portion of air examined; and this fact the author considers supplies the rational explanation of the propagation of the disease without apparent contact with the secretion from the eyes.—*Wochenblatt*, No. 13.

[In No. 19 of the *Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien*, Dr. Schneider gives an account of a new apparatus for analysis of the air, in which he employs cotton-wool in place of glycerine.]—*Medical Times and Gazette*.

X.—ARSENATE OF SODA IN SCROFULA.

Of all the various agents employed in treating scrofula, M. Bouchut has found the arseniate of soda the most efficacious and energetic. Arsenic, in fact, is one of the best of our tonics, and it is a powerful succedaneum of iron, quinine, or cod-liver oil, and for this reason it is efficacious in most organic and nervous cachexiæ when the resulting disorders have not become too considerable or too inveterate. In scrofulous cachexia it is an excellent remedy, children under its influence generally recovering their strength, colour, and appetite. But this is only an amelioration, for in cases in which it effects a cure the cachectic state has not yet been attained, and the local manifestation is confined to the skin, mucous membranes, and the glands. Beyond these, in diseases of bone and in tuberculosis it is only a good palliative. In scrofulous coryza, ulceration of the skin, suppurative adenitis, otorrhœa, leucorrhœa, or perforations of the velum, the relief obtained is prompt. It may be given with quinine or in simple syrup, and M. Bouchut recommends doses of $\frac{1}{10}$ grain of arsenic to commence with, gradually augmenting the quantity.—*Bulletin de Thérapeutique*.

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MEDICAL INTELLIGENCE.

1. *Information for Persons desirous of Entering the Medical Staff of the Army of the United States.*—No person can receive the appointment of assistant-surgeon in the army of the United States, unless he shall have been examined and approved by an army medical board, to consist of not less than three surgeons or assistant-surgeons, to be designated for that purpose by the secretary of war; nor can any person receive the appointment of surgeon in the army of the United States, unless he shall have served five years as an assistant-surgeon, and unless, also, he shall have been examined by an army medical board, constituted as aforesaid.

Boards of medical examiners are convened at such times as the wants of the service render it necessary, when selections are made by the secretary of war of the number of applicants to be examined for appointment of assistant-surgeon. To the persons thus selected, invitations are given to present themselves to the board for examination. These invitations state the time and place of meeting of the board.

Applicants must be between twenty-one and thirty years of age. The board will scrutinize rigidly the moral habits, professional acquirements, and physical qualifications of the candidates, and report favourably, in no case admitting of a reasonable doubt.

The board will report the respective merits of the candidates in the several branches of the examination, and their relative merit from the whole; agreeably whereunto, if vacancies happen within two years thereafter, they will receive appointments and take rank in the medical corps.

An applicant failing at one examination, may be allowed a second, after two years; but never a third.

Applications must be addressed to the secretary of war; must state the residence of the applicant, and the date and place of his birth. They must also be accompanied (*references will receive no attention*) by respectable testimonials of his possessing the moral and physical qualifications requisite for filling creditably the responsible station, and for performing ably the arduous and active duties of an officer of the medical staff.

No allowance is made for the expenses of persons undergoing these examinations, as they are indispensable pre-requisites to appointments; but those who are approved and receive appointments, will be entitled to transportation on obeying their first order.

The pay and emoluments of surgeons and assistant-surgeons are as follows:—Assistant-surgeon, under five years' service, 53 dollars 33 cents. per month. Rations, per month, 36 dollars; forage allowed for one horse, 8 dollars per month. One servant, 12 dollars per month; clothing for servant, 2 dollars 50 cents. per month. Servant's rations, 9 dollars per month. Total amount for servant, 23 dollars 50 cents. per month. Total amount receivable, 120 dollars 83 cents per month.

Assistant-surgeon, over five years' service, 70 dollars per month. Rations per month, 36 dollars. Forage for one horse, 8 dollars per month. One servant, 12 dollars per month. Clothing for servant, 2 dollars 50 cents. per month. Rations for servant, 9 dollars per month. Total amount for servant, 23 dollars 50 cents. per month. Total amount receivable, 137 dollars 50 cents. per month.

Total amount receivable by assistant-surgeon, of over ten years' service, 173 dollars 50 cents. per month.

Surgeons under ten years' service, 80 dollars per month. Rations, 36 dollars per month. Total amount for servant, 47 dollars. Aggregate amount receiv-

able, 187 dollars per month. This includes the allowance for horses and servants.

Surgeon over ten years' service, 80 dollars per month. Rations, 72 dollars per month. Total amount for servants, 47 dollars per month. Aggregate amount receivable, 223 dollars per month.

The allowance for forage and servants is only paid to the surgeons and assistant-surgeons, when they actually employ and keep in service the number of servants and horses charged for.

In addition to the above, surgeons and assistant-surgeons are allowed an additional rate per day after the termination of every five years' service.—*Buffalo Medical and Surgical Journal and Reporter.*

2. We extract the following from the *London Gazette* of Friday, 26th October, 1851:—"Notice is given that the hall of the Faculty of Physicians and Surgeons of Glasgow is recognized by the privy council as an educational vaccinating station for the purposes of the order of 1st December, 1859; and Dr. James Dunlop of Abbotsford Place, Glasgow, officiating as public vaccinator at the said hall, is authorized to give certificates for the purposes of said order to persons whom he at the said hall shall instruct or examine in vaccination."

3. *Perthshire Medical Association.*—A meeting of the members of this association was held in the council-room on Friday, November 29, when the following were elected office-bearers—Dr. Frazer Thomson, president; Mr. Frew, surgeon, vice-president; Mr. J. Bruce Thomson, secretary; and Dr. Bramwell, treasurer. Members of council—Drs. Stirling, Absolon; Laing, Bridge of Earn; and Messrs. Christie and Fleming, surgeons. After the election of office-bearers, Dr. Frazer Thomson, president, delivered an address upon the objects and advantages of such an association for the promotion of medical science and the collateral branches of study; for which able and instructive address the members awarded to the president their unanimous thanks.

BOOKS RECEIVED.

Lectures on the Germs and Vestiges of Disease. By Horace Dobell, M.D., Physician to the Infirmary for Diseases of the Chest. London, Churchill, 1861.

Spinal Debility; its Prevention, Pathology, and Cure. By Edward W. Tuson, F.R.C.S. London, Davies, 1861.

The Principles and Practice of Obstetrics. By Gunning S. Bedford, A.M., M.D., Professor of Obstetrics, &c., in the University of New York. New York, Samuel S. and William Wood, 1861.

Epilepsy; its Symptoms, Treatment, and relation to other Chronic Convulsive Diseases. By J. Russell Reynolds, M.D., London. London, Churchill, 1861.

On the Parasitic Affections of the Skin. By T. McCall Anderson, M.D. London, Churchill, 1861.

A Manual of the Dissection of the Human Body. By Luther Holden, F.R.C.S., Lecturer on Anatomy at St. Bartholomew's Hospital.

A Manual of Psychological Medicine. By John Charles Bucknill, M.D. Lond., and Daniel H. Tuke, M.D. Second edition. London, Churchill, 1861.

Course of Lectures on the Physiology and Pathology of the Central Nervous System. By C. E. Brown-Séquard, M.D., F.R.S. Philadelphia, Collins, 1860.

The Diseases of the Prostate; their Pathology and Treatment. By Henry Thomson, F.R.C.S. Second edition. London, Churchill, 1861.

- A Practical Treatise on the Use of the Ophthalmoscope.** By J. W. Halka. London, Churchill, 1861.
- A Manual of the Practice of Medicine.** By G. H. Barlow, M.D. Second edition. London, Churchill, 1861.
- The Introductory Lecture delivered in the Queen's College, Birmingham.** By John Clay, Senior Professor of Midwifery. London, Churchill, 1861.
- On the Sounds caused by the Circulation of the Blood.** By Andrew Leared, B.A., M.D., Dublin. London, Churchill, 1861.
- On Chloroform and some of its Clinical Uses.** By Charles Kidd, M.D. London, Fieldson and Jary, 1861.
- The Assurance of Diseased and Doubtful Lives on a New Principle.** By Morris A. Black, Actuary, and A. P. Stewart, M.D., Physician to the London and Yorkshire Assurance Company. London, Pateman, 1861.
- Report of a Committee of the Boston Society for Medical Improvement, on the alleged dangers which accompany the inhalation of the vapour of Sulphuric Ether.** Boston, David Clapp, 1861.

IN EXCHANGE.

- Ranking's Half-Yearly Abstract of the Medical Sciences.
- Braithwaite's Retrospect of Medicine.
- The Edinburgh Medical Journal.
- The Edinburgh Veterinary Review.
- The Dublin Quarterly Journal of Medical Science.
- The Dublin Hospital Gazette.
- The Dublin Medical Press.
- The Medical Critic and Psychological Journal.
- The London Medical Review.
- The Medical Times and Gazette.
- The Medical Circular.
- The Archives of Medicine.
- The Ophthalmic Hospital Reports.
- The American Journal of the Medical Sciences.
- The North American Medico-Chirurgical Review.
- American Medical Monthly and New York Review.
- The American Medical Times.
- The British American Journal.
- The New Orleans Medical and Surgical Journal.
- The West India Quarterly Journal.
- The Australian Medical Journal.
- Journal of Practical Medicine and Surgery (Chaillou).
- Journal de la Physiologie de l'homme et des animaux.
- Gazette des Hôpitaux.
- Journal de l'Orient (Constantinople).
- L'Echo Médical, Journal Suisse.

NOTE.—The list of Exchange Journals is only published in our January number. It is earnestly requested that notice of any irregularity in the delivery of the exchange numbers of the "Glasgow Medical Journal" be communicated to the Editors, with precise details of the missing parts.

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